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C. J. Jordan

INVENTOR OF ELECTRO-METALLURGY,

8TH JUNE, 1839.

[*Mechanics' Magazine*, 1839.]

CONTRIBUTION
TOWARDS
A HISTORY
OF
ELECTRO-METALLURGY,
ESTABLISHING THE ORIGIN OF THE ART.

BY HENRY DIRCKS,
CIVIL ENGINEER;
CORRESPONDING MEMBER OF THE LIVERPOOL POLYTECHNIC SOCIETY.
LIFE MEMBER (1837) OF THE BRITISH ASSOCIATION, &c.

LONDON:
E. & F. N. SPON, 16, BUCKLERSBURY.
1863.

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DOMINUS ILLUMINATIO MEA

DEDICATION.

To
MICHAEL FARADAY, ESQ., D. C. L., OXON.; L. L. D., CAM.; F. R. S.; FULLERIAN PROFESSOR OF CHEMISTRY IN THE ROYAL INSTITUTION OF GREAT BRITAIN, &c. &c. &c.

Sir,

From the distinguished position which you hold in connexion with Electrical Science, as well on the continents of Europe and America as in England, I feel that a higher honour than the inscribing of so small a work as the present, is due to you; but permit me to adduce a few, and, as I conceive, cogent reasons for adopting a course which, otherwise, perhaps, might be open to erroneous conclusions.

The history of early inventions, even of many that have most contributed to the necessities and luxuries of life, have been either wholly lost, or continue as matters of unsatisfactory dispute. Of this latter fact the history of the Steam Engine is an eminent example; and that of Electro-Metallurgy was, twenty-four years ago, in an equally fair way to become one of litigation among scientific writers. But as the present work clearly settles the true position of all matters affecting the origin of Electro-Metallurgy, I would beg to notice here, more especially, your early association with that subject.

When the British Association held its first Meeting in Liverpool in 1837, the new, and previously unoccupied building of the Mechanics' Institution in that town, was in a sufficiently forward state to accommodate all the sectional meetings of the Association. The Chemical Section was presided over by you; and at one of its Meetings, Dr. Golding Bird described the simple galvanic arrangement of his invention, which ultimately, in the hands of Mr. Jordan and other experimentors, originated the beautiful processes of Electro-Metallurgy. Dr. Bird's communication deeply impressed you, insomuch that you commented at some length on its details, pointing out, at the same time, what appeared to you as certain discrepancies requiring examination. That address made such a lively impression on my mind at the time, that I even now vividly recollect it, while I thus note the fact.

Your own correspondence also with M. Jacobi, dating as far back as the 21st of June, 1839, afforded you personally an early intimation of the germ of this very art, even in Russia.

With these facts before me, and looking rather to the future than the present, I foresee that, with the growing importance of Electro-Metallurgy in numerous branches of art, the History of its Origin cannot fail to arrest the attention of the ingenious and the learned: so long as it shall be held that, among the greatest benefactors of mankind, must always be ranked that class of men whose genius, as inventors in the arts or the sciences, tends to the progress of society and consequent civilization of the world.

I remain, most respectfully,
Sir,
Your very obedient Servant,
HENRY DIRCKS.

Blackheath, Kent,
January 20, 1863.

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PREFACE.

Nearly a quarter of a century has elapsed since the author's papers on the *History of Electro-metallurgy* first appeared in the *Mechanics' Magazine*, vol. 40, 1839; and nothing has transpired since that period to disturb his original statement in respect to Mr. C. J. Jordan's claim to be the true and first inventor of the art of electro-metallurgy; or, on the other hand, to throw any further light on Mr. Spencer's assumed prior claim: whose letters have necessarily become a part of the history itself. It appeared, therefore, necessary, in publishing the present reprint, not to omit those letters, forming, as they do, so essential a part of the narrative. It is more in justice to Mr. Spencer, than with the most remote desire to re-open the discussion, that this controversial portion is not altogether omitted; because it must be borne in mind that Mr. Spencer by no means withdraws his supposed right to claim rather the *invention* than the mere *improvement* of this most useful art.

As a matter of scientific history, the subject of this volume will gather in interest, in proportion as the value of the science to which it relates increases in importance.

The origin of inventions, at all times interesting, is liable to become involved in difficulties after a lapse of years; but in the present instance all the parties are still living, and such an impartial statement of all that can be advanced in favour of the one side or the other is here given, as must for ever set the matter at rest respecting the origin of the art of electro-metallurgy.

It will be observed that the order of original publication has been preserved—that is, from the first to the fifth paper, in 1844; while the sixth paper, dating 1851-2, gives the key to the supposed disturbing claim that might (but clearly does not) affect Mr. Jordan.

H. D.

BLACKHEATH, KENT,
January, 1863.

INTRODUCTION.

The settling of the claim to priority of invention, whether as affecting their fame or their fortune, or both, is highly important to inventors.

Time was when inventors merged into mythical beings, of whom Vulcan and Mars may, perhaps, be taken as types; but not so in our day. An inventor may now barter his invention, allowing a capitalist or manufacturer to enjoy as his own, the knowledge thus purchased from another; hence inventors have often to contest the market value of their genius. Be it, however, for mere money, or for a nobler aim, the right to priority loses none of its interest.

Hence arises such questions as—Who invented the steam engine? Who discovered the composition of water? Who discovered the planet Neptune? And now, in the present work, Who invented the process called electro-metallurgy?

The Law deals with the monetary question, as in the settling of patent claims. But in such questions as those just suggested, they are only to be settled by *first publication,* which may be—1. By public experiment, or actual manufacture and sale. 2. By a descriptive account delivered before a scientific society; or, 3rd. By a printed statement, put into public circulation.

Whoever, by any of these means, ascertains and makes public the nature of any process or matter of which he claims to be the inventor, must inevitably rank as the true and first inventor.

And no secret, however long it may have been known to have been possessed, can ever take precedence of a published invention; neither can any expressions of intentions to publish, whether verbal or written, or ever so well attested, supercede actual publication; nor can even written communications among private parties obtain precedence of actual printed publication.

The question affects not what might have been done, but what *has been* done, on the part of the alleged inventor.

For an inventor, after considerable study and labour, to be superceded in his ripening discovery by the simultaneous efforts of another party, although a previously unknown and independent inventor, has been no uncommon occurrence. One man has brooded over an invention for years, while another, in a remote quarter, elated with his first success, has, on the same day, become a competing patentee with his dilatory, but unknown rival. The *History of Inventions* affords abundant examples of coincident inventions. And in the instance of electro-metallurgy, or electro-type, it is by no means singular to find Professor Jacobi in St. Petersburgh, Mr. C. J. Jordan in London, and Mr. Spencer in Liverpool—each one unknown to the other—engaged on the same investigation. Known results were preparing the way, awaiting only practical application.

It is quite possible that Mr. Spencer, or some one before him, may have *thought* that the crude markings seen on the deposited copper, as produced by the action of sustaining batteries, might be usefully applied; but if priority of invention had to be settled on such plastic evidence as a man's thoughts, or intentions, or remarks in private conversations, the difficulty of settlement in such cases would be almost hopeless, and generally quite impossible. Therefore it is that publication, whether verbal at meetings, or through the medium of the press, is always admitted as superceding all other evidence whatever, however seemingly plausible or authentic. Were it otherwise, a most pernicious precedent would be afforded for trickery, as in cases where property in an invention might require adjustment; but, independent of such considerations, it is plain that printed publication affords the most clear and undeniable evidence of priority; or could only be disturbed by showing that the unpublished invention differed widely from the published one; and that the supposed first invention was totally impracticable, while the other offered the only true system of operation.

But still further to prove how unsubstantial is the claim to an invention on any other grounds than actual publication, strictly so called, numerous instances might be adduced, and I should have thought it my duty to produce them, had there been any real difficulty to clear up in the present instance. One very analogous case, however, is worth recording.

I allude to M. Le Verrier's discovery of the planet Neptune. It is well established that Professor J. C. Adams, of Cambridge, had calculated the time for the luminous appearance and place in the heavens where it would most probably be seen, which he transmitted to Professor Airy, with a request to have the requisite observations made at Greenwich; but the Astronomer Royal, disputing some part of the calculations, and correspondence passing in consequence, postponed the investigation until it was too late, thus allowing time for an independent, remote observer to reap the honor for France, otherwise due to our own country.*

Here, Professor Adams had done everything—nay, more than Mr. Spencer could show in reference to electro-type; and yet no one doubts but that M. Le Verrier is the true discoverer of Neptune.

By his own acts, or, we may say, it is Mr. Spencer's misfortune, that whatever he claims to have invented in reference to electro-metallurgy bears no earlier publishing date than 12th September, 1839. Whatever transpired *previously* was *private conversation or correspondence*, which goes for nothing as affecting decisions in regard to priority of invention, in the face of evidence of a previous publication.

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* For full particulars, see *Transactions of the Royal Astronomical Society*, 1846, vol. vii.

It may assist the general reader to see the dates and facts herein detailed reduced to a tabular form, as follows:—

9th May, 1839. Mr. T. Spencer wrote a general notice* to the Secretary of the Polytechnic Society, that he was engaged in experiments which appeared to accord with those of Jacobi in St. Petersburgh, noticed in the *Athenæum*, which he would take "an early opportunity of laying before the Society." See pp. 81, 82.

22nd May, 1839. Mr. C. J. Jordan *wrote* a letter to the *Mechanics' Magazine*, descriptive of a process for engraving and obtaining casts by galvanism. See pp. 3—6.

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* In the admirable "*Abridgments of Specifications relating to Electricity and Magnetism*," printed 1859, by order of the Commissioners of Patents, an error occurs in the Introduction at page 78, through stating, in reference to this *notice*, thus—"This paper [alluding to May, 1839] was read September 12, 1839." The notice of May, 1839, and the paper of 12th Sept., 1839, were *two entirely distinct communications*, as diametrically opposite in character as could be. This slight oversight might, if passed unnoticed, hereafter create some confusion; not, however, that it could have weight sufficient to affect the real merit of the case. See this notice at p. 81, and the paper read 12th Sep., 1839, at pp. 86—88.

8th June, 1839 (Saturday). The weekly number of the *Mechanics' Magazine* issued, *publishing* Mr. Jordan's letter, 22nd May, 1839; the earliest published account of electro-metallurgy.

12th September, 1839. Mr. T. Spencer read before the Liverpool Polytechnic Society a paper, entitled "An account of some experiments made for the purpose of ascertaining how far voltaic electricity may be usefully applied to the purpose of working in metal." Pp. 15, 16.

9th February, 1844. Mr. Spencer wrote a letter, promising "documents from well-known persons, collected in 1841, for insertion in the *Philosophical Magazine;*" Mr. Richard Taylor having granted permission. Not yet published, however. See pp. 25, 26.

December, 1851. Mr. Spencer resuscitates his claim, based on his letter of May 9, 1839. Pp. 80—96.

In concluding these introductory remarks, the author very unwillingly ventures to obtrude on the reader's attention a few personal observations, which might have been avoided here by appending them as notes to Mr. Spencer's several letters; he has, however, preferred publishing his own and Mr. Spencer's remarks in their original form. It might appear to some that he had gone out of his way in this matter, whereas his whole life has been devoted to literary and scientific pursuits. To chemistry and to electricity he devoted many years of close study, giving many public lectures on both subjects during several years. But eventually settling in London as a consulting engineer, the subject of inventions became, more than ever, daily a matter of as serious as it was agreeable occupation. He took up the subject of the origin of electro-metallurgy *con amore*, and might never have published more than his first paper, had not the unwarrantable temper of the rejoinders it occasioned obliged him to prove the truth of every point in which the matter could be viewed, until at length, what was intended as a mere plain statement of fact, became a warmly-contested and ultimately successful struggle; his original position being thereby only the better established, to the effect—That PROFESSOR DANIELL is the *discoverer* of the principle on which the deposition of compact copper depends; that Mr. C. J. JORDAN is the *inventor* of the first application of that principle to useful purposes in the arts, known as electro-metallurgy; and that Mr. T. SPENCER is the first of the class of *improvers* of this art so invented.

The author did not rush eagerly, or needlessly, into this enquiry. Four years had elapsed, during which long period it was competent for others to have asserted Mr. Jordan's claims. But the facts with which the papers here given commence must satisfy every impartial reader that it was high time that further misconception should be prevented, by a timely exposition of every possible fact that could be produced. This is all that has been attempted; and it is only to be hoped that the result will prove favourable to him whose cause is advocated: whose genius and truly modest estimation of his own talents are justly deserving of some public acknowledgment.

CONTRIBUTION TOWARDS A HISTORY

OF

ELECTRO-METALLURGY.

In looking over the *Mechanics' Magazine* for several years past, my attention was rather particularly attracted to vol. xxxvi. for 1842, in which appears a paper, entitled, "Books on Electro-Metallurgy,"—a review of the works of Mr. G. Shaw and Mr. A. Smee on that subject. The writer says:—

"On a former occasion (vol. xxxiii. p. 20), we stated our reasons for assigning to Mr. Thomas Spencer, of Liverpool, in preference to every other claimant, the merit of this exceedingly valuable addition to our manufacturing processes; and nothing has since transpired to induce us to modity in the slightest degree the opinion that we then expressed on this head." It is added, "we are sorry to see that there still exists in certain scientific circles the same dogged reluctance, on which we before animadverted, to do justice to the humble 'carver and gilder* of Liverpool,'—for no better reason, than we can discover, but the sin of being humble." And further on remarks, "Be it so, since the fact is so, that it was by 'ACCIDENT' Mr. Spencer made the discovery, still it was an *accident* of that happy sort which happens to but one or two men in an age, and which, by the universal consent of mankind, entitles him to whose lot it falls to be looked upon with all the respect, honour, and gratitude due to the chosen instrument of any great revelation by nature to her children." The only liberty taken with this extract, is that of printing the word "accident" first in capitals, and afterwards in italics. How far Mr. Spencer has been indebted to *accident*, the sequel will show.

The earliest application of galvanic action to a useful and ornamental purpose that I am acquainted with, was practised by Mr. Henry Bessemer, of Baxter House, Camden Town, who, above ten years ago, employed the galvanic apparatus to deposit a coating of copper on lead castings. The specimens I have seen are antique heads in relief, the whole occupying a space of about 3 inches by 4 inches. They have lain as ornaments on his mantelpiece for many years, and have been seen by a great number of persons.

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* Mr. Spencer, as a master carver and gilder, occupying a large house and fine shop, in one of the leading thoroughfares, might have been justly offended at this designation; unless it is merely meant that in certain circles it is deemed humiliating to be of any but the learned professions.

The earliest published account of the manipulation requisite for obtaining casts by galvanic action is contained in the letter of a Mr. C. J. Jordan, dated 22nd May, 1839, and published in the *Mechanics' Magazine*, for June 8, 1839. This letter is so interesting in connexion with the history of electro-metallurgy, that I repeat it here; in particular, I would direct attention to the fact of the main incidents named by Mr. Jordan, published June 8, 1839, agreeing with those published by Mr. Spencer, September 12, 1839; and, curious enough, being called forth by the same vague announcement of Professor Jacobi's experiments, which was then making the round of our periodicals. Both parties describe Dr. Golding Bird's small galvanic apparatus: one used a printer's type, the other a copper coin; and both recommend the application of heat to remove the precipitated copper. The letter is as follows :—

ENGRAVING BY GALVANISM.

"Sir—Observing in the last page of a recent number of your *Magazine*, a notice extracted from the *Athenæum*, relative to a discovery of Professor Jacobi, its perusal occasioned the recollection of some experiments performed about the commencement of last summer, with the view of obtaining impressions from engraved copperplates, by the aid of galvanism, which led me to infer some analogy in principle with those of the Russian Professor, and may probably give me the right to claim priority in its discovery and application. These experiments were abandoned from the want of that most important element in pursuits of this nature—time; the writer's share of the said element being occupied in a manner more imperative than pleasing. I regret, however, not having made it the subject of an earlier communication, as this would have placed my pretensions beyond doubt; but inasmuch as the notice alluded to is given from memory, and is undescriptive, while I may be enabled to exhibit the *modus operandi*, my assertion may be at least partially substantiated.

"It is well known to experimentalists on the chemical action of voltaic electricity, that solutions of several metallic salts are decomposed by its agency, and the metal procured in a free state. Such results are very conspicuous with copper salts, which metal may be obtained from its sulphate (blue vitriol) by simply immersing the poles of a galvanic battery in its solution, the positive wire becoming gradually coated with copper. This phenomenon of metallic reduction is an essential feature in the action of sustaining batteries, the effect, in this case, taking place on more extensive surfaces. But the form of voltaic apparatus which exhibits this result in the most interesting manner, and relates more immediately to the subject of the present communication, may be thus described:—It consists of a glass tube, closed at one extremity with a plug of plaster of Paris, and nearly filled with a solution of sulphate of copper; this tube and its contents are immersed in a solution of common salt. A plate of copper is placed in the first solution, and is connected by means of a wire and solder with a zinc plate which dips into the latter. A slow electric action is thus established through the pores of the plaster, which it is not necessary to mention here; the result of which is the precipitation of minutely crystalized copper on the plate of that metal, in a state of greater or less malleability, according to the slowness or rapidity with which it is deposited. In some experiments of this nature, on removing the copper thus formed, I remarked that the surface in contact with the plate equalled the latter in smoothness and polish, and mentioned this fact to some individuals of my acquaintance. It occurred to me, therefore, that if the surface of the plate was engraved, an impression might be obtained. This was found to be the case, for on detaching the precipitated metal, the most delicate and superficial markings, from the fine particles of powder used in polishing, to the deeper touches of a needle or graver, exhibited their correspondent impressions in relief with great fidelity. It is, therefore, evident that this principle will admit of improvement, and that casts and moulds may be obtained from any form of copper.

"This rendered it probable that impressions may be obtained from those other metals having an electro-negative relation to the zinc plate of the battery. With this view, a common printing type was substituted for the copperplate, and treated in the same manner. This also was successful; the reduced copper coated that portion of the type immersed in the solution. This, when removed, was found to be a perfect matrix, and might be employed for the purpose of casting, where time is not an object.

"It appears, therefore, that this discovery may be turned to some practical account. It may be taken advantage of in procuring casts from various metals, as above alluded to; for instance, a copper die may be formed from a cast of a coin or medal, in silver, type metal, or lead, &c., which may be employed for striking impressions in soft metals. Casts may probably be obtained from a plaster surface surrounding a plate of copper; tubs (tubes?) or any small vessel may also be made by precipitating the metal around a wire, or any kind of surface, to form the interior, which may be removed mechanically, by the aid of an acid solvent, or by heat.

"C. J. Jordan.

"May 22, 1839."

I was aware of Mr. Jordan's letter at the time of its publication, and have frequently been surprised since, that his name has not transpired in any discussion I have heard on the subject. Nothing certainly can be clearer than his reasoning, the details of his experiments, and his several concluding suggestions.

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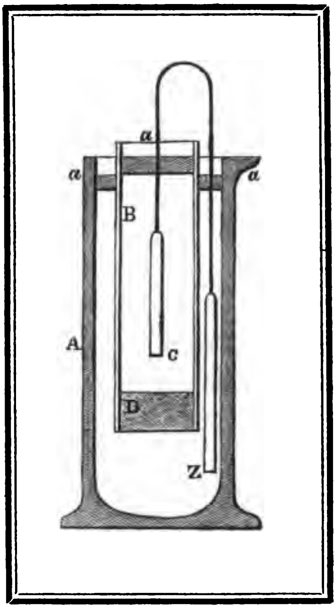

It was particularly in September and October, 1837, that several parties attached to scientific pursuits in Liverpool, were engaged in repeating the experiments of Dr. Golding Bird, published in the Phil. Trans. for 1837, and of which he gave an account before the chemical section of the British Association at its meeting in Liverpool, over which Dr. Faraday presided. The apparatus used on that occasion by myself and others, was precisely that recommended by Dr. Bird, consisting simply of any glass vessel A, capable of holding a solution of common salt, into which is inserted a gas-lamp chimney, having its lower end plugged up by pouring into it plaster of Paris D; a solution of sulphate of copper is then poured into it, and the whole immersed in the contents of the glass A, and tightened with pieces of cork *a*, *a*, *a*; C is the copper, and Z the zinc plate. The result expected from this arrangement was the deposit of metallic veins of copper within the plaster diaphragm, independent of any connexion with the poles [of the battery. Dr. Faraday and every other electrician expressed surprise and doubts at the results in this respect, said to have been obtained by Dr. Bird, and Dr. Faraday particularly urged the necessity and importance of caution in receiving as established a result so greatly at variance with all former experience, and proceeded to explain a variety of causes tending to lead to fallacious results in this curious and interesting experiment.

I have still by me the crystals of copper obtained by myself in my first experiments in September, 1837. I was at that time a good deal in communication with Mr. John Dancer, philosophical instrument maker, Liverpool (now of the firm of Messrs. Abraham and Dancer, Manchester), and this was a frequent subject of conversation. It was about October of the year following (1838), that Dr. Brett was delivering lectures on electricity and galvanism at the Medical Institution, Liverpool, and being in communication with Mr. Dancer respecting the apparatus for those lectures, I was arranging for a supply of galvanic troughs, when Mr. Dancer showed me a ribbon of copper, thin but very firm, granular on one side, while it was bright, and smooth, all but some raised lines, on the other. Seeing my curiosity a good deal excited, he said at once, it was the result of galvanic action, and explained the whole process. He particularly noticed that generally the deposit was more crystalline, granular, and brittle. The difference of the specimen I was examining, which possessed all the tenacity of rolled copper, he attributed to his having gone to the potteries to look out suitable jars for forming sustaining batteries, and having fixed on a lot which he was told would not answer, as *they were not glazed,* and would not hold liquor. The idea immediately occurred to him that such *unglazed* jars might be turned to account and used instead of bladder, brown paper, plaster of Paris, and other porous substances he had previously employed. He, therefore, immediately obtained a sample for experiment, and the result was a more firm and compact deposition of copper, though that was not what he was seeking, his object being durability and equable action. But Mr. Dancer was not negligent of the accidental discovery of a deposit possessing such tenacity as belonged to the copper resulting from batteries, in which he employed porous earthenware; neither did he make any secret whatever of the occurrence. A specimen of the copper he obtained by that process was shown by me the same day to Dr. Brett, in the laboratory of the Apothecaries' Hall, Liverpool, who was equally unable with myself to guess the mode by which it had been obtained. When informed of the process, he was much gratified by the statement.

These facts were named by Mr. Dancer's friends to various persons, and in my capacity of Honorary Secretary, at the time, of a Literary and Scientific Institution, this curious electrical experiment of Mr. Dancer's was frequently a subject of conversation.

Subsequently, Mr. Spencer laid claim to the discovery of the means for obtaining metallic casts by galvanic agency, and having, with others, expressed doubts of Mr. Spencer's claim to priority, I was surprised when verbally informed by Mr. Spencer, that I was quite in error in repeating such statements, and that so far from its being the fact, Mr. Dancer had "made the *amende honorable.*" This statement, which appeared to me a very extraordinary one, induced me to write a note of inquiry to Mr. Dancer, and the following is his reply :—

"21, Pleasant Street,
Liverpool, 17th June, 1840.

"DEAR SIR—Since I saw you I have scarcely had ten minutes to spare, and at present I have not time to enter fully into detail. With respect to the precipitation of copper for the purpose of obtaining an impression, I give you the following account (part of which I believe you were aware of in 1838), and is, in fact, the substance of a letter written by me in reply to Mr. T. Spencer. I stated that I had been engaged, in connexion with Mr. Unsworth, in experiments with batteries of various constructions, previous to my acquaintance with Mr. Spencer, and I had used various porous substances in my batteries (which were those excited with common salt and sulphate of copper). To prevent the intermixture of the solutions—in one I had used a thin plaster division, and I obtained a very compact deposition of metallic (copper) on the copper plate of my battery, which I pulled off. I recollect Mr. Unsworth and I talking of it as one method of multiplying our battery plates, while we were making use of the battery. From this I was led to try if I could precipitate a larger and more uniform plate, and I succeeded in doing so. I afterwards thought that unglazed jars, such as I could obtain at the Pottery, would be more durable for the porous division, and, on trial, I found them answer very well; with these I could obtain the copper more uniform.

"I met Mr. Spencer one morning in Berry Street, and I happened to have one of these precipitated copper plates with me, which I showed to him. When I told him how it had been formed, he would scarcely believe it, until I pointed out the impressions in relief, of all the minute scratches that were on the plate, against which it had been deposited. The surprise that Mr. Spencer expressed very naturally led me to suppose that it was the first compact piece of precipitated copper that he had seen. I believe it was the same which I sometime after gave you, and which you showed to Dr. Brett. I had also reversed* the experiment of Dr. G. Bird, using the solution of copper outside, and I placed a piece of sheet copper at the bottom of the glass, and in connexion with the zinc which was in the gas glass; on the copper I fastened, by means of varnish, a letter D, cut from a printed bill. The copper precipitated on all parts of the plate except where the letter was fixed; when I peeled the precipitated copper off, the letter came out, not having any connection with the outside edge. I also obtained an impression by stamping my name on a copper cylinder, the impression being the reverse way. All these experiments were well known to persons about my place, and are well recollected by them and all who were in the habit of calling. You will recollect no doubt the many conversations we had respecting the deposition of copper. All this happened many months before I was aware that Mr. Spencer had been engaged in anything of the kind, except that he had Dr. Bird's experiment in action. Sometime after this, Mr. Spencer applied to me for one of my porous jars, and one evening at his house he told me the purpose for which he wanted it,—he stated that he intended to form ornaments by gilding patterns of plaster and depositing copper on them; he stated also, that it would be of great use in his business. The same evening I told him of my having obtained impressions by stamping my name on a copper cylinder.

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* This remark of Mr. Dancer will be immediately understood by a reference to the diagram before given, which is just the reverse of his description.

"I saw a notice in the *Liverpool Mercury*, May, 1839, to the effect that Professor Jacobi was engaged in producing fine lines on copper by means of galvanism, and that Mr. Spencer, of Liverpool, had been engaged in the same manner.

"You will recollect that when Mr. Spencer brought the subject before the Polytechnic Society,* that you and others also, that knew I had been experimenting in the precipitating of copper, expressed great surprise that I did not make some mention of it. The reason I gave you at the time was, that being acquainted with Mr. Spencer, I thought it would look like envy, or a wish to detract from the merits of his experiments, and share the honour without having brought it to any practical use; but as he has stated to you, and others, that I had made the 'amende honorable,' which insinuates that in the letter I sent to him (to explain the grounds on which I stated that I had showed to him the first compact copper by precipitation that he had seen), I had been retracting, or making some apology, for what I had stated—you will be able to judge from this statement of undeniable facts whether I have been misrepresented.

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* He gave notice of his paper 8th May, but did not bring it forward till 12th September, 1839.

"The whole of the matter may be summed up thus, —I never did, nor ever wished, to take credit for Mr. Spencer's experiments; but if he had, as he now states, produced compact precipitated copper at the time when I showed him the piece in question, he was wrong in allowing me to suppose otherwise; and that whether he had or not, the experiments that I tried originated with me in the manner described, and this is all I have ever desired to maintain.

"Yours truly,

"Jno. Dancer.

"Henry Dirks, Esq."

Mr. Thomas Spencer read his paper "*On Voltaic Electricity applied to the purpose of working in metal,*" before the Liverpool Polytechnic Society, on the 12th September, 1839. He purposed making this matter the subject of a paper intended to have been read at the meeting of the British Association, in August, at Birmingham. He had announced to the Polytechnic Society on the 8th May, that he had a communication of this nature to make, and he did so at that time, and in that manner, in consequence of a paragraph appearing in the public prints, that Professor Jacobi was engaged on experiments in engraving by galvanic action. The same year the Society printed Mr. Spencer's paper for gratuitous circulation. Mr. Spencer, in this paper, claims to have been engaged on the subject of it for two years, dating, in short, from September, 1837, the very period when Dr. G. Bird read his paper at the Liverpool meeting of the British Association. He says, without the least acknowledgment of the apparatus being the identical ingenious one then suggested by Dr. Bird, "In September, 1837, I was induced to try some experiments in electro-chemistry, with a single pair of plates, consisting of a small piece of zinc, and an equal sized piece of copper, connected together with a wire of the latter metal." It is unfortunate for the reputation of Mr. Spencer, that while the Society was at the expense of publishing his paper to uphold his claims, he should make that very paper the vehicle for underrating and undervaluing not only the labours of others, but actually the labours of experimentalists to whom he was indebted for the germ of his own experiments in electro-metallurgy. The "humble 'carver and gilder of Liverpool,'" designates the learned lecturer at Guy's Hospital, "a clever young demonstrator (Dr. Bird, of London)," and speaking of Dr. Bird's experiments before alluded to, this same "humble" individual very complacently observes, in reference to obtaining crystals of pure copper within the plaster diaphragm of the apparatus, "I doubted this at the time, as it was opposed to all former experience." Not a word that Dr. Faraday had spoken at considerable length on this very point — not denouncing it as a decided error, but suggesting with that delicacy of remark and philosophical acuteness of observation for which he is remarkable, various reasons for further enquiry and consideration. Mr. Spencer at one dash takes to himself the merit of singular shrewdness of observation in this matter. Mr. Dancer had produced an indent, in the shape of a D, on the deposited copper, where a letter, cut out of paper, had been attached by varnish to the copper plate. Mr. Spencer details, as a discovery of his own, indents on the deposited copper, where some sealing wax varnish had dropped on the copper plate.

It shows a singular want of observation and ingenuity on the part of Mr. Spencer, a carver, and claiming acquaintance with friends, "some of whom are connected with the public press," that he should not have seen, or did not try to obtain, a wood block engraving previous to making the laborious experiment of trying to obtain a copper plate for printing, with lines in relief "one-eighth of an inch in thickness!" when a mere hair line would have sufficed.

Mr. Spencer experienced great difficulty to avoid the brittleness of the copper deposit, and tried plaster of various thicknesses, and solutions differing in strength, in all of which he seems to have displayed considerable perseverance; and, which, besides what he sought, led him to the discovery that by a clean surface the coating of copper became adhesive; and the reverse, if not freed from grease, or oxidation, by employing a solution of nitric acid.

Mr. Spencer's paper is divided into two portions. The first refers to executing engravings, the second to forming moulds and castings of medals, ornaments, &c., and are said to have all been made simultaneously, that is, between Sept., 1837, and May, 1839. Whether any improvements between May and Sept., 1839, are introduced, is not stated; neither do any letters or affidavits accompany the communication. Specimens illustrating the progress of the experiments from first to last were exhibited to the Society 12th Sept., 1839, showing some of the early specimens, of a very brittle and useless character for any purpose in the arts; and here it was that Mr. Spencer received a most valuable hint from Mr. Dancer, whose experiments were not only freely communicated to him, but his very apparatus obtained from him by Mr. Spencer; yet without the least acknowledgment then, or since, of having acted otherwise than by his own unaided judgment. Yet we see that the first promptings were given at the Liverpool meeting of the British Association, by Dr. G. Bird's simple and very ingenious little galvanic apparatus—that the scientific journals were rife with discussion on applications of galvanism to useful purposes of the arts—that Mr. Jordan, a correspondent of the *Mechanics' Magazine*, in the plainest and most intelligible terms, explains identically what Mr. Spencer has a little, and but very little elaborated. Lastly, therefore, that, through the *Mechanics' Magazine* (which Mr. Spencer was regularly taking in), the experimental results obtained by Mr. Dancer, and the reports, in April and May, 1839, in the public papers, of Jacobi's experiments, all bring such broad hints and abundant assistance to aid Mr. Spencer, that he is rather to be praised for his application to the extension of what was already known on a smaller and less perfect scale, than to be adjudged a discoverer, much less "the father of electro-metallurgy," having a "preference to every other claimant." He commences his paper by saying, "I do not profess to have brought forward a perfect invention." He should rather have said, "*I do not profess to have brought forward a perfect* improvement of what has already been crudely suggested by others."

It is a remarkable fact that Mr. Spencer has made no useful or profitable application of the electrotype process, of which his first experiments gave promise; neither did he early secure its applications by patent right, patenting only, at an after period, a portion of the process, of little or no benefit to him. The consequence has been that he has been left immeasurably in the background. It does not say much for Mr. Spencer's possession of originality of genius and philosophical acumen, which a perusal of his paper on voltaic electricity would persuade us he considers belongs to him, so long to have remained an absolute cypher in an art entirely new, and capable of modifications and applications yet untried.

The facts now set forth have slumbered near seven years, and are only now given lest they should be entirely lost. I have every possible respect for Mr. Spencer for what he *has* done; but common candour obliges me to own that I consider that he has only followed in the footsteps of others, repeating their experiments with some little improvement; but certainly neither at the first originating a new art, nor afterwards doing much to add to its resources, or promote its progress.

London, January, 1844.

[To this letter, Mr. C. J. Robertson, the then editor (but since deceased) of the *Mechanics' Magazine*, appended the following remarks:—

"Mr. Dircks has proved beyond all doubt that we have made a great mistake in advocating so strenuously the claims of Mr. Spencer to the invention of electrography. No one, however, can suppose that we would intentionally exalt any one, at the expense of our own journal, which we are now pleased to find was the honoured medium of the first distinct revelation of this important art to the public, by an old and esteemed correspondent of ours, Mr. Jordan. Whatever Mr. Bessemer, Mr. Dancer, or Mr. Spencer, or others may have previously said or done, it was in private—made no secret of perhaps, but still not communicated to the public at large—not recorded in any printed work for general benefit. For anything previously done by any of them, the art might have still remained in the profoundest obscurity. No published description of an earlier date than Mr. Jordan's can, we believe, be produced; and when we look upon that description, it is really surprising to see with what fulness and precision the writer predicated of the art, nearly all that has been since accomplished. In supporting, as we did, the claims of Mr. Spencer, to be considered as the first discoverer, we had lost all recollection of Mr. Jordan's communication. We had no personal acquaintance with either of the gentlemen, and could have no motive for favouring one more than the other. We took up the cause of Mr. Spencer with spontaneous warmth, because we thought him to be a person most unfairly and ungenerously used; as in truth he was, so far as the intention went, by those who having at the time none of the reasons we now have, for questioning Mr. Spencer's pretensions, yet obstinately refused to acknowledge them. If it should seem to the reader more than usually surprising that Mr. Jordan's paper escaped the recollection of the Editor, through whose very hands it passed to the public, his surprise will be lessened, perhaps, when he observes, how it appears to have also escaped the notice, or been passed over in silence, by every one else down to the present moment—even of those, not a few, who have expressly occupied themselves with the history of electrography—Mr. Noad, for example, whose very candid and impartial work we reviewed in our last number, and who specifies the *Mechanics' Magazine* as one of the publications which he consulted in its preparation. To us the most surprising thing of any connected with the case is, that neither Mr. Jordan himself, nor any of his friends, should before now have thought it worth while to vindicate his claims to the promulgation of an art which justly entitles him to take a high place amongst the benefactors of his age and country.—Ed. *M. M.*]

I.

MR. SPENCER,

In support of his claim, wrote as follows:—

Sir—My attention was drawn to an article in your *Magazine* by the following paragraph, which appeared in the *Liverpool Albion* of Monday last:—

"ORIGIN OF ELECTROTYPE: MR. SPENCER NOT THE DISCOVERER.

"An elaborate paper, being a 'Contribution towards the History of Electro Metallurgy,' is just published in the *Mechanics' Magazine* of the present week: it is by Mr. Henry Dircks, of London, formerly of this town. Mr. Robertson, the respected and talented editor of that magazine, was, until the present time, a warm advocate of Mr. Spencer's claims; but, so convinced is that gentleman by Mr. Dircks' statements and arguments, that he falls in entirely with his views. Mr. Dircks gives, in succession, the names of Mr. H. Bessemer, Mr. C. Jordan, Mr. John Dancer, and only in the fourth place does he find a niche for Mr. Spencer. We imagine this paper will make some stir among scientific circles, there never having appeared, hitherto, much certainty as to whether or not Mr. Spencer was the original inventor, or a mere copyist. We really think Mr. Dircks' paper must be taken as confirmatory of the latter opinion."

I at once guessed Mr. Dircks was its author, having already had ample opportunities afforded me of this gentleman's peculiar talent, in thus drawing attention to his own productions; although I confess, in this instance he speaks in less flattering terms of himself than usual. On personal enquiry at the office of the *Albion*, I found my surmise still further confirmed.

I quote this paragraph, although not strictly relevant to my present purpose, that your readers may know something of the character of the man who has endeavoured, through the medium of your pages, to heap charges on me, little, if anything, short of dishonesty of purpose. No one acts up in argument to the trite saying "that two blacks do not make one white," more than I do; I shall therefore say the less of Mr. Dircks. Still, I cannot help expressing regret that I have not a better opponent in this matter, or at least one of whose upright intentions in the cause of truth I was better assured.

I regret that you should have formed so hasty an opinion with respect to my claims as you seem to have done, in your remarks appended to Mr. Dircks' article.

Had I not had the power of reply, nor a friend to make it, still there was enough in the article itself, not to go farther, to have, if not suspended, at least modified it.

Do not for a moment mistake me; I speak not in the spirit of recrimination, and shall ever feel grateful for the warm manner in which you advocated what you considered were my just claims, and can easily conceive how the same warmth of feeling should have led you into an opposite direction, when you considered that, to a certain extent, you had been deceived; but what I contend is, that had Mr. Dircks' statements been fully borne out, they did not quite warrant the conclusion you arrived at.

The main object of my present communication is to say, that my hands are at present full, and shall be so for the next three or four weeks; I shall then have time to string together some documents from well-known persons, which were collected in 1841, and which, I trust, will prove beyond all doubt, not only that I have never laid claim to more than I was entitled to, nor even as much, but that with respect to the discovery of electrotype, I was first in the field, and first in all subsequent improvements connected with it. This may be deemed egotism; but, Sir, there are times when a man must speak out, and the present I judge to be one of them. The documents I speak of as being collected in 1841, were for the purpose of insertion in the *Philo-*

sophical Magazine, Mr. Richard Taylor being written to for permission, which he very kindly gave; but business interfered, and I thought as all seemed to acknowledge my claims, that it would perhaps be considered impertinent on my part, to thrust testimonials before the public in corroboration of that which they did not deny—so it fell through.

Still I have longed for a fair opportunity of publishing them, which I think is now afforded me. One of these letters is from Mr. Dancer himself, in which the following sentence occurs: "I never did, to my knowledge, state that I had given you the first hint of your experiments." I cannot help thinking, that as respects myself, I was justified in deeming this an "*amende honorable.*"

I now request permission to insert the whole of the documents in question in your magazine, along with such reply and explanations as may occur to me. They are far from being voluminous, and I shall make what I may think it necessary to add as short as possible.

Mr. Dircks would have it inferred, that because I have not got rich with the electrotype, *ergo*, I am not the inventor. This gentleman is a very Nadgett* at worming into other people's circumstances with respect to pecuniary matters—it is one of his besetting sins. I thought, however, that the rule was, that an inventor did not get rich with his own invention, and I had comfortably set myself down as not being destined to form any exception.

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* See Boz.—[Note by Mr. Spencer.]

Again, Mr. Dircks would argue, that because I have not followed the invention up, and have allowed others to take precedence of me with respect to it, that that affords another probability against me. But while I deny the legitimacy of both conclusions, in the latter instance I utterly deny the fact; on the contrary, I know of no signal improvement connected with the discovery which I have not promulgated; and, as something like evidence of this, I send you but a very brief, I may say imperfect, abstract of a paper read by me on the 10th of last October, at the Liverpool Polytechnic Society. This abstract, as you will perceive, was printed in the *Liverpool Standard* of the 17th of the same month. The paper itself was a voluminous one, and the numerous experiments connected with it required both time and strict attention. You will perceive it involves the practical application of a new principle, as connected with electrotype, and I know of nothing in the whole range of electrical science that presents us with such a beautiful variety of experiments, both philosophically and practically. The paper itself was destined for the *Philosophical Magazine,* and permission had been obtained for its insertion; but my rough notes require polishing, and this is far more labour to me than a whole range of experiment; and it required illustration with wood-cuts, of which I must have furnished the drawings. While slowly engaged with this, the sudden death of a parent occurred; it was then laid aside: however, I hope shortly to publish it, as at first intended. As delays are dangerous (I speak, as you may admit, from experience), I should be glad if you will insert this abstract in your columns. Nor is this the only labour of mine that has not found its way out of the columns of our Liverpool papers, which, were it not for the reporters connected with them, would not even have appeared there.

I am, dear sir,
Yours respectfully,
THOMAS SPENCER.

Liverpool, February 9, 1844.

[To this communication the late Mr. Robertson appended the following note:—

"We are much obliged to Mr. Spencer for the abstract of the paper referred to at the conclusion of his letter, but it would be contrary to editorial etiquette, and even common literary justice, to anticipate, in the way he proposes, an article which he had previously promised in full to another journal. With respect to the further, and more satisfactory reply to Mr. Dircks, which he requests permission to insert in our pages at some future day, he is entitled to the permission, and we readily grant it. To every thing by which he can make good his title to the invention of electrography, once so earnestly and conscientiously advocated by us, our pages shall always be freely open. For the present, we can but regret that Mr. Spencer should have thought it right to have forborne, from any cause whatever, even for a single day, that complete vindication which the statements of Mr. Dircks seemed to call for at his hands. —Ed. *M. M.*"]

CONTRIBUTION TOWARDS A HISTORY

OF

ELECTRO-METALLURGY.

(CONTINUED.)

In the former part of this paper, I introduced to notice Mr. H. Bessemer, of whom I have but a very slight personal knowledge; also Mr. C. J. Jordan, *the inventor of Electrography,* who is totally unknown to me; thirdly, Mr. J. B. Dancer, an ingenious electrician, with whom I have been long on terms of intimacy; and finally, Mr. Thomas Spencer, of whom I know very little beyond having occasionally met him in public. The tendency of my remarks is obviously such as to persuade every impartial reader, on the evidence adduced, that Mr. Spencer has hitherto maintained a very false position, merely owing to the singular circumstance of his unjustifiable claims never having before been contested. The consequence has been not only the leading of our own scientific writers into error, but likewise those on the Continent; thus the Royal Academy of Paris affords, in an elaborate memoir on the subject, read last year, an eminent example of the consequence of neglecting the earlier adjustment of this claim to discovery. Since the publication of the first part of my historical sketch of the metallurgic art, Mr. Spencer has thought proper to publish letters in the *Mechanics' Magazine* and *Liverpool Albion*, quite in the style of one who is *Gratis anhelans, multa agendo nihil agens*. It is impossible I should here avail myself of those letters, as they contain nothing but abuse and misrepresentations,* and the very strange subterfuge, that his "hands are at present full, and *shall* be so for the next three or four weeks." Half the time and space occupied in writing a letter containing matter to the purpose would have produced a letter which would have been willingly inserted by all the leading journals throughout the kingdom. But Mr. Spencer being immersed in mesmeric experiments on a Mrs. Tod, a Scotch woman, and which has led to his reading a paper before the Liverpool Literary and Philosophical Society on certain phenomena of *clairvoyance*, most injudiciously allows such pursuits to take precedence of what is at present a far more urgent and important engagement, his offering immediately those proofs of his rightful claims to a disputed discovery, which he would have us believe he possesses, and has had prepared since 1841.

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* As examples:—I am charged by him in the *Liverpool Albion*, with "dishonesty of purpose;" but what he may mean by this I know not. In the *Mech. Mag.*, Feb. 17., p. 107, he upraids me with inferring, that because he has "not got rich with the electrotype," and again, because he has "allowed others to take precedence of him," in carrying it out, *ergo*, that he is "not the inventor." I warn Mr. Spencer to keep to facts. I defy him to find any such inference, as he here states, in my paper of last January, published Feb 3. It is a poor cause that requires any resource short of a strict adherence to truth; and surely Mr. Spencer must have known these were direct falsifications when he penned them; else he is an unpardonably hasty and inconsiderate writer.

I had originally intended to have confined my remarks to my first paper, but this extension of the subject, relating to Mr. Spencer, is called for and invited by his recent assertions. To set myself right with the public, and prove the absence of any under-current of private motives, I desire to observe at the outset, that I am not aware of having seen Mr. Spencer for the last three years (certainly not above once); I have never been in any way indebted to him, or he to me, and have no unfriendly feeling towards him personally. I have taken up this matter solely from its having been so long unnoticed by others. The only personal affront Mr. Spencer can be said to have offered me must date from his letters within the last fortnight, but which I willingly excuse.

I shall now, without waiting for the advantage of any thing Mr. Spencer may be reckless enough to bring forward to support his assumed rights, proceed at once to lay bare such an example of plagiarisms as is perhaps without a parallel in the whole history of science, when honour, more than emolument, has been the object of contest. It is requisite, however, that I should early fortify my position, by unravelling a little the mystery under which Mr. Spencer has somewhat ingeniously, and with success so far, enveloped all that appertains to his proceedings.

It will be recollected that he announced to the Liverpool Polytechnic Society, May 8, 1839, that he had a process of applying galvanic agency for effecting the deposition of metals, which announcement he made fearing he should be anticipated, rumours being afloat respecting Professor Jacobi's galvanic engraving. It might have been supposed, that from Sept., 1837, when he claims to have first commenced experiments in electro-chemistry, to, say June 1839, would have afforded him time sufficient to lay before the Society a very fair outline of his announced discoveries, *if any were really made.* But no—neither then, nor in June, was the Society favoured with his communication—a Society of which he was a member, and to which, in case of claims to priority, he meant to fly for proof of his announcement. The "common carver and gilder" (for Mr. Spencer claims to be nothing more)* could rest satisfied with no channel of communication short of the British Association, to meet in Birmingham the August following. It was thus he made a convenience of the Polytechnic Society to secure his supposed rights, and sacrificed to his ambition his own interests—(supposing him to have had some real and honest interests to protect)—to appear before the assembled philosophers of Europe! How he missed attaining the summit of his wishes—how his proffered revelation was most uncivilly declined—he has himself related—and those who are curious to know all the particulars may refer to his own narrative. On the 12th September, 1839, the revelation of Mr. Spencer's much-vaunted discoveries, declined by the British Association, was permitted to be made to the Liverpool Polytechnic Society; which, at a public meeting on the 13th Jan., 1841, handsomely awarded him a prize of ten pounds for the subject of his communication.

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* *Mechanics' Magazine*, vol. xxxiii. 1840, p. 492.

The *Mechanics' Magazine* was foremost to take up Mr. Spencer's cause; no eulogy that could reasonably be bestowed on him was spared; quotations illustrating the warm advocacy of that long-established and widely-circulated journal in his favour occur in the first part of this paper. In vol. xxxiii. for 1840, we find him, at page 208, associated with Professor Jacobi, in an abstract from a Munich paper; at page 324, Mr. W. A. Kentish pays him a similar compliment; at page 373, Mr. Spencer's pretensions afford the principal portion of extracted matter in a review of Mr. W. Savage's Dictionary of the Art of Printing; while at page 491, the editor's honest warmth of feeling for depressed and unappreciated genius (as he then supposed) knows no bounds short of proposing that "A grant of a few thousands to so successful a contributor to the arts of peace as Mr. Spencer would garland the way, by which the losses of many wars might be repaired!"

Turn we now, after a lapse of little more than three years, to his letter in the *Liverpool Albion* of February 12, in which he very coolly and deliberately delivers himself after this manner—"I have just read the article in the *Mechanics' Magazine* (Feb. 3), and I shall find but little difficulty in making a satisfactory, and, I trust, final reply to it. In all probability I shall do this in the pages of the magazine in question; or, possibly, in one of a higher character (!), in order that this part of the question may be set at rest. I only wish, however, I had a better man to refute than Mr. Dircks." (!) It must have been in much the same spirit that he at first estimated the British Association as an institution *better* calculated to establish his fame than (as he would consider) a merely provincial Polytechnic Society; in a like strain too he here distinguishes our scientific journals, and can actually speak of a *better* one than the *Mechanics' Magazine*, under the offensive insinuation of his possibly patronising "one of a higher character." Supremely quixotical, indeed, is his questioning of the writer's character; but the same sentence offers a grave charge against him of deep ingratitude for speaking slightingly of a journal which, more than any other, has contributed to direct public attention to his labours; to maintain his supposed rights; to publish his productions; warmly to second the claims urged by himself; always his apologist and friend; and which has to the uttermost endeavoured to promote his welfare, with all that benign and enthusiastic spirit which is ever ready to extol and foster genius—particularly if supposed to be labouring under difficulties.

As far as regards the mystery of Mr. Spencer's successful usurpation of Mr. Jordan's place, that has perhaps been sufficiently accounted for. Another mystery remains to be noticed; it is that of the wonderful accumulation of coincidences of which Mr. Spencer has been the ill-fated victim! To do more than enumerate them here would be almost an act of supererogation; those chiefly alluded to are:—

1st. That of carrying out usefully voltaic action to working in metals, *just after* the publication of Mr. Jordan's process.

2nd. That of obtaining compact copper instead of the usual friable and brittle quality, *just after* Mr. Dancer had shown him the first specimen obtained by his new plan of porous earthenware jars.

3rd. That of his theory and practice of soldering metals, some time *after* the French patent of M. de Richemont's autogenous soldering, using jets of flame produced by air and hydrogen. (See both in the *Mech. Mag.*,* vol. xxxii., 1840, p. 546, and p. 729; and vol. xxxiii., pp. 44, 126, 262.)

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* In an editorial note it is observed—"Mr. Spencer, however, while

4th. The circumstance of his inventing corrugated copper plates for the jar batteries, *just after* Mr. Dancer had contrived and made several on the same principle.

5th. His suggesting a new view of heat in a paper on the subject of atmospheric electricity, read before the Liverpool Literary and Philosophical Society, long *after* it had been published, and the doctrine he claimed as original and his own had been discarded.

No one acquainted with the particulars brought to light so far in my present and former communications, can peruse attentively Mr. Spencer's paper on the electrographic process, read before the Liverpool Polytechnic Society, and which next comes under consideration, without detecting the spirit of detraction and determinate appropriation that incessantly obtrudes itself.

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thus asserting his own claims to originality, very frankly and honourably adds, that he has no doubt whatever (!) M. Richemont made the discovery first, and that he has no wish or intention to interfere with the rights of the English Patentee." Mr. Spencer had on the 14th May, 1840, read a paper on the subject to the Liverpool Polytechnic Society; and very complacently boasts of his having announced it to the Society "as far back as the 18th February," 1840, all which, till we know the facts, must be considered very fair and straightforward. And so it has been considered, with no small share of praise to Mr. Spencer for his apparently urbane conduct! But what shall we say of Mr. Spencer, at the very time a subscriber to, and reader of, the *Mechanics' Magazine*, when it is known in connexion with the foregoing, that in vol. xxx. of the *Magazine*, on 30th March, 1839, page 480, is given (nearly twelve months before his announcement) a notice of M. D. de Richemont having invented his apparatus for soldering lead, called "aerhydric pipes," with the nature of his process? The English patent for the same was sealed July, 1838.

He has studiously endeavoured to appear as standing alone, originating all the experiments he has adduced, prompted only by the suggestions peculiar to a philosophically inclined genius. Finding, however, that he must work with some apparatus, not of his own invention—make applications, in which he was tremblingly conscious he had been anticipated by Mr. Jordan, and attempt to vary applications which he had barely more than the talent to multiply, his course was considerably circumscribed; wherefore, he has proceeded as if he had hoped that past experiments, and the results obtained by others, would be lost by blazoning forth his repetitions of those experiments as his own discoveries, which, at the worst, he could but yield up when claimed. Yet, as if to let no chance escape of establishing his title to the highest claims of being a discoverer, he seems to have thought it requisite to reduce, as much as lay in his power, the importance of published opinions by suitable inuendoes; and, above all, to avoid stating any point capable of being construed into a favourable opinion of the labours of others, or as distinguishing his own knowledge of their labours in the same field.

Now for the proofs that these are more than merely vague charges. I shall show the plagiarisms of his first published paper.* There is no doubt Mr. Spencer claims originality, otherwise he would not give his experiments, as he declares, for "illustrating the progress of discovery," p. 9; or speak of "the present simple discovery," p. 10; or of not having produced "a perfect invention," or of imagining he has "discovered a useful application of an important principle," p. 11; or commend the subject of his paper as "the present simple discovery," p. 10.

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* A pamphlet published under the title of—"*An Account of some Experiments made for the purpose of ascertaining how far Voltaic Electricity*

It was in the *Athenæum* for May, 1839, that he first saw a notice of Professor Jacobi having discovered, in the words of the paragraph, "a method of converting any line, however fine, engraved on copper, into relief by a galvanic process."* In consequence, on the 8th May, 1839, as was before stated, he announced to the Liverpool Polytechnic Society, that he should make a communication to them of his process for effecting the same or similar results. But, as we have seen, it was not until the months of August and September that he

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may be usefully applied to the purpose of working in metal. By Thomas Spencer. Liverpool, Mitchell, 1839, 8vo., p. 26. Printed for gratuitous distribution."

* The following is the paragraph alluded to, and which also appeared in the *Mechanics' Magazine*, May 11, 1839. "*Galvanic Engraving in Relief.* While M. Daguerre and Mr. Fox Talbot have been dipping their pencils in the solar spectrum, and astonishing us with their inventions, it appears that Professor Jacobi, at St. Petersburgh, has also made a discovery which promises to be of little less importance to the arts. He has found a method—if we understand our informant rightly—of converting any line, however fine, engraved on copper, into a relief, by galvanic process. The Emperor of Russia has placed at the Professor's disposal, funds to enable him to perfect his discovery.—*Athenæum.*"

declared himself prepared to divulge his great secret. And what was it, after all, that he had discovered up to May 8, 1839, allowing him the utmost latitude, on his own representations? Why, not galvanic engraving; not Professor Jacobi's process; etching it might be called, but engraving it certainly was not, his method being to coat copper with etching ground, scratch in a few lines, and make a voltaic deposition of copper, affording lines in relief most assuredly, but lines essentially differing from the lines in relief taken from those "engraved on copper." Mr. Spencer confined the operation to lines engraved, or more properly etched on varnish, laid on copper (pp. 13, 14). It is very probable that by a hurried or mistaken reading of the original newspaper paragraph (see the note) he had acquired, and was misled by, a wrong intimation of the method of following out the hint afforded by that announcement. Mr. Jordan proceeded more ingeniously when, in the early part of the summer of 1838, he produced electrotypes from true "engraved plates," affording, as in Professor Jacobi's experiments, "lines in relief." I cannot refrain from here noticing the marked propriety of Mr. Jordan's style over the swelling pomp of Mr. Spencer's inflated language. Mr. Jordan refers to the *Athenæum* paragraph, and his own experimental researches in galvanism, applied to engraved copper plates, "which (he says) lead me to infer some *analogy* in principle with those of the Russian professor." Mr. Spencer, who ill refrains his contempt for Professor Jacobi, just reverses the foregoing mode of comparison, and says, "I accordingly concluded that *he* was engaged in experiments *analogous* to my own," p. 7.

Before, however, Mr. Spencer could proceed one step, he required some kind of galvanic apparatus; the one he describes at page 12, is repeated at pages 13, 14, and he refers to the same at page 19, as that "already described;" nay, his pamphlet opens at page 3, with two diagrams showing the identical battery. Mr. Spencer, as he cannot claim to be the inventor of this highly ingenious and truly simple apparatus, we are prepared (knowing he depreciates all that is not his own) to be informed on the first notice of his having used it, that he should only—"mention this experiment briefly,—not because it is *directly* connected with what (as he proceeds to say) I shall have to lay before the Society, but because, by a portion of its results, I was induced to come to the conclusions I have done in the following paper." Now, I say, without fear of contradiction, that *that* apparatus and those experiments were the origin of the whole superstructure of electro-metallurgy, as well in the hands of others, as of Mr. Spencer, of which it is vain attempting any proof to the contrary. Why then treat the matter as so trivial, why endeavour to detract from its value? The answer is plain, the reason twofold. Mr. Jordan, his rival in the same field, whom he had to humiliate, had used Dr. Bird's apparatus,* and spoken of it most justly as "the form of voltaic apparatus which exhibits this (the electro-type) result in the most interesting manner, and relates (he says) *more immediately to the subject of the present communication.*" And what better fate could be expected to attend the much envied author of that very neat galvanic arrangement? When he is named, it is not to do him honour by stating that fact, but to affect superiority by ridiculing him for a very possible error in prosecuting the examination of galvanic phenomena; at the same time designating the learned lecturer at Guy's Hospital as "a clever *young* demonstrator (Dr. Bird, of London)!"—p. 12.

Mr. Spencer, no doubt, constantly dreaded a powerful opponent appearing in Mr. C. J. Jordan, whose letter of May 22 was published June 8, 1839, entitled "Galvanic Engraving," three months prior to the date of Mr. Spencer's; and when the letter of the one, and the pamphlet of the other are placed in juxtaposition, we trace that similarity of design and execution which surely must be more than the result of sheer accident. Mr. Spencer falls into a long train of coincidences, if such they be, though the natural inference is, that they are premeditated, the offspring of the careful, studious, labour of that long period of mysterious silence from May 8 to September 12, 1839. Mr. Spencer of course endeavours to outstrip Mr. Jordan in every point (save the date of publication), and he has a parallel instance for whatever Mr. Jordan adduces of any consequence, so that he cannot even inform us, speaking of the deposited copper, "that the surface in contact with the plate equalled the latter in smoothness and polish," which he "mentioned *to some individuals of his acquaintance;*" but Mr. Spencer must furbish up a story reaching beyond the summer of 1838, and says, "It is two years since I began to experimentalize on this subject (September, 1837). I *then* made mention of it *to a few friends* (some of whom are connected with the public press in Liverpool), but strictly enjoined them not to make it public until the experiments were matured," page 7. Mr. Jordan, in his communication, records the fact of metallic reduction being "an essential feature in the action of *sustaining* batteries." Mr. Spencer, as he could not introduce so important a remark with any feature of novelty, takes another course, and makes no account of it. "I had long been aware," he says, "of what every one who uses a *sustaining* battery with sulphate of copper in solution must know,—that the copper plates acquire a coating of copper from the action of the battery*; but I had never thought of applying it to a useful purpose*

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* For a full account of this elementary battery, see the original paper, "Observations on the Electro-chemical influence of long-continued electric currents of low tension. By Golding Bird, F.L.S., F.G.S., &c." Read Feb. 2, 1837. Phil. Trans., 1st Part, 1837. He observes: "To M. Becquerel we are almost entirely indebted for our knowledge of the chemical agency of feeble currents in reducing certain refractory oxides to the metallic state."

before," p. 13. The natural enquiry, then, that suggests itself on hearing his concluding remark is, whether he has not designedly omitted adding, "*before reading Mr. Jordan's letter?*" That seems to be the truth.

On another important head Mr. Spencer has been somewhat troubled by Mr. Jordan's prior publication, wherein he says of the precipitation of copper, that it will be "in a state of greater or less malleability, according to the *slowness* or *rapidity* with which it is deposited." As this is too important a point to give up, we are apprised, at the second page of Mr. Spencer's paper, of what is to follow, when he says, in his very first description of the apparatus, "It was intended that the action should be slow; the fluid in which the metallic electrodes were immersed were in consequence separated by a thick disc of plaster of Paris," p. 12. Yet at page 15 he informs us, "*I discovered* that the solidity of the metallic deposition depended entirely on the *weakness* or *intensity* of the electro-chemical action," which he could regulate "by the thickness of the intervening wall of plaster of Paris."

Mr. Jordan had found, on separating a precipitated plate from an engraved copper plate, that it exhibited "the most delicate and superficial markings, from the fine particles of powder used in polishing, to the deeper touches of the needle or graver." Mr. Spencer having obtained an impression of the medal, says of the electrotype, "On examination with a magnifying glass, I found every line was as perfect as the coin from which it was taken," p. 19. Mr. Jordan doubts not "casts and moulds may be taken from any form of copper." We find Mr. Spencer "was desirous of executing metallic ornaments by this means, in either cameo or intaglio;" and says of his first essay, "I accordingly determined to make my first experiment on a very prominent copper medal," p. 19. In one experiment Mr. Jordan says, "a common printing type was substituted," by which he formed "a perfect *matrix*, which might be employed for the purpose of casting." Mr. Spencer, in the introduction, or "Prefatory," as he pedantically terms it, to his pamphlet, acquaints us that, "In the formation of that important implement in the manufacture of printing types—the *matrix* or mould—advantages in the adoption of this operation appear (!) to present themselves," page 9. Mr. Jordan very ingeniously observes of the process he describes, that "It may be taken advantage of in procuring *casts* from *various* metals;* for instance, a copper *die* may be formed from a *cast* of a *coin* or *medal*, in silver, type metal, or lead, &c., which may be employed for striking impressions in *soft metals*." Mr. Spencer appropriates this hint, as too natural and too common to be other than general property, and warns us so accordingly. "Being aware," he says, "that copper in a voltaic circuit deposited itself on lead with as much rapidity as on copper, I took a silver coin, and put it between two pieces of clean sheet lead, and placed them under a common screw press," page 20. This experiment of "striking impressions on soft metals," succeeded entirely. Mr. Spencer, in his "prefatory" remarks, lauds the art for "supplying as it does a means of procuring a *cast* or a *die* in hard metal," p. 8; and afterwards for "the applicability of this process in procuring exact fac-similes of *coins* or *medals,*" p. 9; and at length declares, "So complete do I think this latter portion of the subject, that I have no hesitation in asserting that fac-similes of any *coin* or *medal,* no matter of what size, may be readily taken, and as sharp as the original," p. 20. But had not Mr. Jordan said all this before him? Mr. Spencer, p. 21, says, "I now come to the conclusion of my experiments. I took two models of an ornament, one made of clay, and the other of *plaster of Paris;*" one he bronzed, and having gilt the other, he obtained metallic casts from both. Mr. Jordan had before plainly thrown out the hint; and strange enough it happens to be the last suggestion adopted by Mr. Spencer, though it is the last but one in Mr. Jordan's letter, where he says, "Casts may probably be obtained from a *plaster* surface."

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* Mr. Spencer varies the reading of this suggestion thus:—"Nor (I need hardly observe) is its application confined to copper only." Prefatory, p. 8. Why "need hardly observe"? except that it had been quite as well observed before him; but then it was by his rival! Every opinion and process of his own, however dull and blundering, is carefully detailed.

Mr. Jordan's concluding proposal appears to have baffled Mr. Spencer, and to have been too uncouth or unmanageable to have excited his attention, most likely arising from the complexity consequent on a misprint (as it appears) of "tubs" for *tubes*, thus, "tubs or any small vessel, may also be made by precipitating the metal around a wire, or any kind of surface, to form the interior, which may be removed mechanically, by the aid of an acid solvent, or by *heat*." The metallic coating on wires would evidently form *tubes*, which Mr. Spencer has thus missed the lucky opportunity of proposing for gas pipes, and recommending to the gas companies! Who can say but it was while thinking how to make *tubs* that he struck out the magnificent idea of coppering ships' bottoms (as he must be familiar, in a seaport town, with the calling of any old vessel a "*tub*"), thus literally supposing "tubs, or any small vessel," to be synonymous terms; and which, oddly enough, may actually have led to his preparing the galvanic-coppered model, which was afterwards sent by him to the Admiralty!

Mr. Jordan, as just noticed, recommends the application of "*heat*," as one means of removing the deposited metal, but Mr. Spencer ingeniously gets hold of the same idea by finding out that when a plate is varnished to deposit the copper in the clean engraved lines, that on heating the plate to remove the varnish, the whole network of copper came along with it, p. 15. Having thus made himself, in this also, an undoubted *discoverer*, and got rid of Mr. Jordan's assistance, he proceeds—"When I wished to take it (the electrotype) off, I applied the *heat* of a spirit-lamp to the back;" and again—"When the *heat* of a spirit-lamp was applied for a few seconds to the lead, the copper impression fell off easily," p. 20.

The apologist for Mr. Spencer might pretend to show that discoveries depend on accumulated facts, that all discoveries are made up of what, in some way or other, was already known. With such men it is impossible to argue. It is not pretended that a discourse wants originality, merely because every word of it may be found in a dictionary. What has Mr. Spencer brought forward of consequence in his paper of September 12, that is *not* found in Mr. Jordan's of May 22, published the 8th of June, that can entitle him to take precedence as a discoverer, or inventor? Nothing. We may allow him, and justly, the praise of effecting some *improvement.* I believe I state correctly, in awarding him the merit of suggesting the use of nitric acid to give the copper-plate a clean surface, when adhesion of the deposited copper is required; the pointing out and distinguishing the effect of any foreign matter, however slight, to prevent adhesion; even pencil lines on a copper-plate having this effect; the using of an etching ground to attach raised lines to the copper through the etched lines (the present beautiful process of glyphography); the engraving of sheet lead to obtain copper-plates with a surface of the nature of wood engraving; and, probably, the substitution of bronzed or gilt plaster, &c., for metallic casts. Mr. Spencer did *not* go farther, at that period, unaided by the suggestions of Mr. Jordan. I venture to say, that there is no one who has read Mr. Jordan's letter but will allow that, had Mr. Spencer's writing *never* appeared, electrography would have flourished by following Mr. Jordan's most excellent, clear, and full prescriptions.

Mr. Spencer, shortly after the publication of his foregoing paper, became the compiler of a treatise entitled "*Instructions for the Multiplication of Works in Metals by Voltaic Electricity,*" published by Messrs. Griffin and Co., of Glasgow, 1840, a review of which appeared in the *Mechanics' Magazine,* vol. xxxiii., p. 491. In that pamphlet occurs the very kind of argument Mr. Spencer might be expected to reiterate in extenuation of his unlimited borrowing and appropriating of other men's works and opinions, after passing them rather through a sieve than refining them in an alembic. He says, as if preparing an argument to exonerate himself from any future charge of plagiarism, "Few who watch the progress of events can doubt that, had this discovery not been made at this present period, a very brief additional time would have brought it under public attention. (!) Scientific facts were all tending towards it. The great discoveries of the last age were being condensed and combined into the elements of learning for this. Trains of scientific thought, which had been long and curiously laid (!), only required the aid of the match (! !) to explode them simultaneously." The reviewer, led away by the apparent "truth and modesty" (to quote his words) of Mr. Spencer's statements, commits himself to the following eulogy:—"What have all our most valuable discoveries and inventions been but happy thoughts—but matches suddenly applied to long pre-existing trains." He likewise remarks, "Mr. Spencer did not patent his discovery, but made at once a free gift of it to his country and the world!" Mr. Spencer knew more than the reviewer—he knew better than to patent a recorded discovery, an invention not his own, and wisely confined himself to patenting what he afterwards, perhaps, did discover—a simple branch of its application; his right to which claim, however, I am informed, is, curiously enough, at present a matter of legal contest against him.

Throughout the present attempt to clear up the extremely involved details illustrating the origin of this truly novel, interesting, and extensively useful branch of art, I have been conscious that there is some risk of my appearing in the unenviable light of being actuated by other than the golden rule of giving honour to whom honour is due. Truth being my only aim, I see not why I should succumb to the phantom fear; trusting, as I do, to the right guidance of honesty of purpose, regardless of consequences under such direction. This second part of my paper I have no alternative but to publish: it has, I have shown, been invited by most unmeaning and uncourteous insinuations on the part of Mr. Spencer, which I trust I have proved to be groundless.

His taunt of being able easily to set aside my statements has also induced my going more into detail. To have noticed earlier his unfounded claims, it was thought, would have interfered with some pecuniary interests he might possibly have in the matter. Absence, together with many pressing engagements, farther prolonged the period of my attention to this part of the subject, until it was forced on my notice by the circumstance first named, of an accidental reference to the volumes of the *Mechanics' Magazine*. Every plea for continued silence has, after a lapse of four years and a half, entirely subsided: were I to except, indeed, an intimation that I may expect, for any protest entered against his invalid claims, a heavy shower of abuse from the subject of this memoir—of which the small dark cloud does, indeed, already appear in the horizon. But, in vindicating the cause of Mr. Jordan, is not the cause of justice and of science equally blended? Have I advanced one vulnerable point which can cast the claims of Mr. Jordan into the shade, to the resuscitating of Mr. Spencer's perishing fame? And if Mr. Spencer can, as he asserts, overturn my strictures, is it not highly blameworthy his delaying to unfurl his promised banner of victory?

If the writer may now be allowed to say a word for himself, he would observe, that in what he has written he has no personal motives of any kind whatever to serve, other than, as now, taking all justifiable means to show that he was prepared with full and sufficient evidence for whatever he has advanced. The facts stated have long been before his mind, and in choosing the present time and manner of making them public, he considers it fortunate his doing so during the lifetime, he believes, of all the parties interested: than which he can offer no better proof of not shrinking from test of enquiry. It is likewise his intention, at a future period, to give in another form his present views of the history of electro-metallurgy, when he would not wish to stand alone, as at present, differing from all preceding writers: it is, therefore, desirable to have the matter early set at rest. If Mr. Spencer feels aggrieved, of which his latest letters certainly afford evidence, he should seriously reflect that, as his laurels are being stripped from his brow, it behoves him to occupy that time in vindicating his now disputed claims, which he is infelicitously wasting in maligning one who will be foremost, on the successful issue of such an event, to atone as best he can for (in that case) a great wrong done to a great genius.

II.

MR. SPENCER,

In support of his claim, wrote, in continuation, as follows:—

Sir—In a note appended to my last communication, you express regret that I should have let anything interfere with an immediate reply to Mr. Dircks. Well-meant as that advice was towards myself, it was contrary to that of my friends who read the attack, and certainly to my own opinion, until, at least, I was at full leisure to reply. Nor did I deem his statements, with regard to myself, of such high importance as to require any undue haste. I think, however, you will now admit I was not wrong in pursuing this line of conduct. By referring to Mr. Dircks's last contribution to the Magazine, it clearly appears that he had not made the whole of the charges one would have thought he was bound to do, at the time he published his first paper.

Whenever Mr. Dircks says his case is complete—or, in other words, when he confesses to have brought before the public all the facts or opinions he may be in possession of, with reference to myself, and the discovery of electrography—I shall then feel bound to think seriously of replying to his statements. At present, however, I have neither time nor inclination to keep up a mere war of words with him, either through the medium of your own columns or those of a newspaper. Take the following as an example of what this sort of thing would lead to: —Let us suppose that which is not very wide of the truth,—that Mr. Dircks makes statements which I know to be unfounded in fact. Say, then, I reply by a denial. Mr. Dircks, however, in your next, either reasserts them, or presses something fresh, taking no notice whatever of the denial. Why, Sir, a fertile imagination, combined with a dash of mendacity on the part of the accuser, might even tire the patience of the accused, to say nothing of your readers.

But why go to hypotheses? Here are two cases taken from your last week's number, which are neither more nor less than simple falsehoods.

Mr. Dircks says, I entertained the idea that ships' bottoms might be coppered by the electrographic process, and that I sent a model vessel, so covered, to the Admiralty. The avowed intention of this is, to convey that I must have possessed but a very imperfect knowledge of the subject when I was capable of entertaining an idea so absurd. So, indeed, I confess I would. It does so happen, however, that my opinions are on record, where I ridiculed such an idea when proposed to me. In addition to which, *I did not send a model, so covered, to the Admiralty* I have long since put amateur ship-builders in the way of covering the bottoms of their toy vessels with copper; but at a meeting of the

Polytechnic Society I devoted part of an evening to show the utter absurdity of such schemes on a large scale. So much for case the first. Now for the second.

Mr. Dircks states that my right with respect to a patent in connection with electrography is at present matter of legal contest against me. To this I think it will be unnecessary to say more than, if such indeed be the case, I am in blessed ignorance of the fact—a state of things, you will admit, by no means usual with a defendant under such circumstances.

I could quote a few more of a similar nature from last week's Number; but suppose Mr. Dircks chose to repeat that the above—trivial as they are—are true (which is by no means improbable), am I bound either to let judgment go by default, or to apply to the Admiralty for written authority to prove the negative on the one hand, or to make inquiry in the proper law-courts to ascertain whether there is anything pending against me on the other? I put it thus to show its absurdity.

Unless, therefore, Mr. Dircks makes a fixed statement, which he will bind himself to keep to—friend Dircks being what we call in Lancashire "a slippery customer" —and inform us when it is complete, I do conceive I am not bound to answer until then; more especially when he informs us he intends to publish his ideas on the subject in another form—in all probability in the shape of a pamphlet, which may be transmitted through the post-office; nor does he put this contingency as to whether he is refuted in his statements or not. I should therefore, under the cirumstances, be noways surprised, even should the whole of these charges be successfully rebutted, to find Mr. Dircks get HIS "*contributions towards a history*," as he facetiously, no doubt, terms them—printed almost *as they stand*, and sent gratis—of course he could not hope for a sale—to all quarters where he thought my reply would not be met with. To a certain extent he is now doing so, with those Numbers of your Magazine that contain his own papers. As a sort of wholesale check to our friend's mania for a one-sided diffusion of knowledge, I make the following proposal; and, if he is the honest friend of truth he takes such pains to tell us he is, he can have no possible objection to it. First, then, that he shall make out the whole of his case through the medium of your pages, or say if he has already done so. After which I shall make a definite reply in the same periodical. The whole case thus put shall be submitted to a committee of gentlemen of acknowledged competence to decide how far either party has sustained his case. Second, should Mr. Dircks wish to reprint his contributions on this subject, that he shall agree to append to them the decision of the committee, be it what it may.

I am, Sir,
Yours very respectfully,
THOMAS SPENCER.

Liverpool, February 27, 1844.

CONTRIBUTION TOWARDS A HISTORY

OF

ELECTRO-METALLURGY.

(CONTINUED.)

It will be seen from the foregoing letter that Mr. Spencer required the completion of any statements I might have to make before clearing himself of the charges brought against him. His patience, under any other circumstances, would be highly commendable; but, in the present instance, while taunting others with "slipperiness," he, at least, should have avoided what he so properly condemns. I can, however, well understand that, in the unenviable position in which he must feel himself placed, he will, by the judicious portion of his friends, whose counsel he follows, be recommended to adopt the tortuous course of special pleading, of which he has given us an example in his wordy letter of the 27th Feb. All this wily artifice aids in gaining time, besides the chance it affords of thereby entrapping an opponent. A dubious kind of credit is, therefore due to Mr. Spencer's friendly counsellers in a case requiring on his part the utmost caution, so that by following the course adopted, he at least acts prudently. Still it would be advisable for him to consult further as to the propriety of continuing this course of Chancery practice, especially as the veritable *discoverer of electrography* is himself prepared to step forward to vindicate his rights. I grant Mr. Spencer must have been in a state of trying uncertainty as to this fact, for it would assuredly have made somewhat in his favour had his rival either been in a foreign land, or still further removed to "that bourne from which no traveller returns."

Mr. C. J. Jordan has hitherto been an entire stranger to me; it was only last Wednesday, the 28th Feb., that I had the pleasure of a first interview with him. I find he was born in London, and is by profession a printer. He is a young man of very unassuming manners, possessing much ingenious inventive talent, as well as a thorough knowledge of electrography, of which he is the rightful originator. That he takes precedence of Mr. Spencer, I have fully proved; Mr. Spencer would, therefore, only act justly to make honourable mention of a man whose published labours were mentioned to him in 1839 by myself, in a communication I made to him of their existence in the columns of the *Mechanics' Magazine*, but of which he then made no account; yet of Mr. Jordan's labours (as I have shown) he has amply availed himself, though his very *name* he has, to the present moment, pertinaciously persevered in suppressing—a degree of caution singularly significant of the weakness of his cause.

Mr. Spencer finds, in my last paper, two "simple falsehoods," as he unceremoniously terms them; and the naming of these, he seems to think, at once invalidates my whole evidence. Of the first charge, I beg to say I am sorry I should have placed over-much reliance on my authority, having ventured the statement on an assurance made to me in a very positive manner by Mr. Spencer *himself*, that he had transmitted to the Admiralty *a model vessel, coppered by the electrotype process.* Mr. Spencer here, with admirable *naïveté*, presents in himself an amusing instance of a person of limited information swelling into all the importance of a very oracle, when he dogmatically says, "I have long since put amateur ship-builders in the way of covering the bottoms of their toy vessels with copper; but at a meeting of the Polytechnic Society I devoted part of an evening to show the *utter absurdity of such schemes on a large scale.*" And very wroth is he with my statement; assuring us that "the avowed intention is, to convey that I must have possessed but a very imperfect knowledge of the subject when I was capable of entertaining an idea *so absurd.* So, indeed, I confess I would!" For Mr. Spencer's information, it is a fact, in spite of that other fact that HE had "devoted part of an evening to show the utter *absurdity* of such schemes," that the Lords of the Admiralty actually have had constructed for some time past large tanks, capable of floating full-sized boats, and which have been coppered therein by voltaic action. Ergo, on his own "confession," we must allow Mr. Spencer to possess *"a very imperfect knowledge of the subject!"* It is lamentable to see the *soi-disant* discoverer of electrography thus affording ample evidence of the fallacy of even his most confident and arrogantly expressed opinions. The gist of my ironical remarks had reference not so much to the coppering of vessels, as to the probability of the idea springing from the coppering of "tubs!" Of the second charge, I can only say, that having prefaced it by the words, *"I am informed,"* (the statement as to his patent being a matter of legal contest against him), it is evident from this that I did not give it my own authority, and that I, at least, am not chargable with wilful misrepresentation. Mr. Spencer must surely be aware that all this stickling at trifles looks very suspicious, particularly while he leaves the main charge unattacked.

I have to thank Mr. Spencer for the opportunity now afforded me of rectifying an error committed in my last paper, which, as it flattered his vanity, I suppose might otherwise have passed unnoticed. I have since found that others have committed the very mistake I wish to correct. In my enumeration of his *improvements*, I name his "using an etching ground to attach raised lines to the copper through the etched lines (the present beautiful process of glyphography)." The glyphographic process is the reverse of this, being one by which an entire electrotype plate is produced, the varnished and etched copper-plate being merely employed to serve as a kind of mould.

In the Glasgow *Practical Mechanics' Magazine* for February 17, appears an article on electro-metallurgy, in which the writer says, "We may mention here, that during the same time that Spencer was maturing *his discovery*, so as to bring it before the public, Professor Jacobi, of St. Petersburg, announced a similar discovery, and, as in many other instances, the merit of the *first discovery* of this art has consequently become a matter of dispute, both claiming precedence. From the evidence brought forward on the different sides, it would be *a matter of some difficulty to decide to whom it most justly belongs*," p. 164. It is thus, as I hint in my last paper, that the prevailing misconception respecting Mr. Spencer's pretensions runs through every scientific work on the subject at home, and which, unless corrected, we need not be surprised at the error being perpetuated abroad.

Mr. Spencer, as if nothing would serve but making him "bare as the palm of the hand," pretends to suppose I have not yet completed my charges against him, merely because in my second paper I endeavour to strengthen what I had advanced against him in my first paper, in consequence of his jeeringly designating my remarks as "this little attack of Mr. Dircks's." In my first paper I asserted his plagiarisms; in my second I prove their existence. Having intimated his want of originality, I prove in my second contribution, by five examples, that we have abundant evidence of his building his fame on the merit due to others. He may be right in supposing the subject not exhausted; no doubt a reference to the French and German scientific journals, with which he has of late made himself acquainted, would lead to the true source of many of *his* discoveries (?). But I have no ambition to scrutinize the amount of originality belonging to his other papers, not even his last elegant production on *clairvoyance** (detailing Mrs. Todd's powers of second sight), particularly after the specimen of gross plagiarisms already so clearly made out, in respect to his paper of September 12, 1839, and which he so evidently shrinks from refuting.

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* In his late letters Mr. Spencer places together two very incongruous remarks—1st, That he will have *little* difficulty in overturning my statements, and yet, 2nd, That his having his *hands full*, he claims a few weeks' respite. The *Liverpool Journal*, of March 2, throws some light on what has so much absorbed his valuable hours. No less than four columns of the journal are occupied with a verbatim report of Mr. Spencer's paper "*On some Recent Instances of Clairvoyance*," read at the Royal Institution, Liverpool, before the Literary and Philosophical Society, on the 19th of February. All the instances given refer to Mrs. Isabella Todd, a Scotchwoman, and is a clever puff of Mr. Spencer's shop and laboratory, of which she gives a very fair inventory, even to his very "papers full of scribbles," to use her own significant term. The intellectual treat afforded by this good woman's conversations might well satisfy him to lie awhile in the dust. Take one example, which shall suffice, though there is abundance of the same edifying dialogue:—"I then asked what was on the walls." "Two shelves." "What is on them?" "Dishes." "What is on the floor?" "Some

Mr. Spencer entirely misunderstands me in my concluding remarks. All I meant to convey was, that in any future notice of the history of electrography, in a premeditated electrical treatise, I should acknowledge only Mr. Jordan as the discoverer, and *not* Mr. Spencer, as has hitherto most mistakenly and unjustly, though I am sure very unknowingly, been done by every preceding writer. And Mr. Spencer, much as he plumes himself

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dishes." "What is on the table you spoke of?" "Papers full of scribbles." Mr. Spencer's own share in the business was such as might well make his "hands full," and the Liverpool Phreno-Mesmeric Society are fortunate in having such an advocate as Mr. Spencer, over all whose performances there presides the semblance of an imposing genius. One illustrative example of Mr. Spencer's experiments must close this note. "After being in the adjoining room (he says, Mrs. Todd being in the clairvoyant state in another apartment) some minutes, perfectly still, I put my thumb to the tip of the nose, and extended the fingers outwards, in the most approved manner, and as we frequently see enacted by the clown in the ring. I remained in this dignified attitude for some time. I then returned to the other apartment, and requested to know what I had been doing, the parties present having been instructed to question her during my absence. I was answered by some tittering, but Mr. Todd said that she was no doubt exhausted by the length of the experiments, and they could not get her to answer what I had been doing." The whole of this verbose paper is nothing more than a tedious enumeration of vulgar commonplace experiments, which are made the medium for conveying, first, to a learned scientific body, and afterwards to the public, the highest order of phenomena affecting the animal economy; yet he most unsatisfactorily winds up by confessing, "I have no theory to offer."

on a happier fate, will nevertheless find that his sun has set, and that whatever of honour can attach to being the inventor of the electrographic process, will hereafter be unhesitatingly awarded to Mr. Jordan. *Palmam qui meruit ferat.*

In connexion with the subject of the invention of electrography, I think the following extract will prove interesting, from a paper "On the Effects of a Battery charged with Sulphate of Copper," by Warren De la Rue, in the *London and Edinburgh Philosophical Magazine* for 1836. He says, "The zink plate is always partially covered with a coating of copper, which, however, is not detrimental to the power of the battery; the copper-plate is also covered with a coating of metallic copper, which is continually being deposited; *and so perfect is the sheet of copper thus formed, that, on being stripped off, it has the polish, and even a counterpart of every scratch, of the plate on which it is deposited.*" Mr. De la Rue makes this remark incidentally, and I only offer it here as a proof how near others have trenched on the discovery of a useful application to the arts of that very deposited copper, which was a result unfavourable to the action of the battery. Mr. Jordan was the first to direct attention to a useful and ornamental adaptation of this property, peculiar to the action of sustaining batteries, literally bringing good out of evil—for it was before always a desideratum to get rid of this *detrimental* deposit.

[To this communication, Mr. Robertson, the Editor, appended the following observations:—

"We cannot blame Mr. Dircks for not subscribing to the terms which Mr. Spencer wishes to have conceded to him before entering on his vindication from the charge of misappropriation which has been brought against him. Mr. Spencer is in no condition to stipulate terms. The charge has been made, and it is for him to answer it, or not, as he pleases—it is his own affair. If he do not, the public will know what conclusion to draw. We fear that, as it is, any denial he may now make will come all too late to prevent impressions very much to his prejudice. It was an easy thing to say, 'When I claimed the English invention of electrography, I knew nothing of Mr. Jordan's letter; I had never seen it, never read it, never heard of it;' and if he could say so truly and sincerely, we cannot understand why he should have deferred doing so for a single instant. Mr. Spencer has written two pretty long letters to us on the subject since the charge was brought against him, but we have looked in them in vain (much to our regret) for any such distinct disavowal as this. People do not ordinarily take time to consider whether they have been guilty of such offences as that imputed in the present case. It would not have taken Mr. Spencer a hundreth part of the pains to say the few words we have suggested, which he has taken to evade, or at least postpone the saying of them.—Ed. *M. M.*"]

CONTRIBUTION TOWARDS A HISTORY OF ELECTRO-METALLURGY.

(CONTINUED.)

HAVING in my former paper alluded to my meeting with Mr. Jordan, I shall here introduce the letter he addressed to the Editor of the *Mechanics' Magazine*, it being so confirmatory of all I had stated, independent of any previous personal acquaintance:—

"SIR—The very flattering manner in which my claims in connexion with the discovery of electro-metallurgy have been recognized in your pages, demands from me a grateful acknowledgment. When my communication of May 22, 1839, appeared in your *Magazine*, it was submitted to public consideration to avert, as far as possible, a loss of precedence impending on further delay in its publication, coupled with the conviction, that if worthy of attention it would receive it in the end. Although it would be untrue to deny that disappointment was felt at the neglect it met with, yet, to have murmured at the tardiness of public judgment would have been a step without immediate object. Reflecting also, that in whatever light my letter may have been viewed, nothing advanced therein had been disputed, I determined to remain silent on the matter, and wait the arousing of public attention by other hands—a consummation which I foresaw must, in some way or other, ultimately take place. The expectation has been realized, and I have been indulged with the accession of a vigorous advocate in one of your ablest correspondents, whose contributions to a "History of Electro-metallurgy" have set the matter in a much clearer light than I feel myself qualified to have done. Whatever may be the relation of this gentleman with other individuals mentioned in his paper, with myself it is purely impersonal. The comprehensive exposition of Mr. Dircks appears to have been dictated solely by an impartial spirit, and a just perception of the merits of the case with a view of eliciting truth. In return for his valuable exertions in my cause, I beg to express my sincere thanks.

"It may be necessary to mention here, that at the time of the publication of the second 'Contribution,' &c., its author was personally unknown to me; but that in consequence of a conversation with Mr. Brown, librarian to the Mechanics' Institution, I was induced to open a communication with Mr. Dircks, which led to an interview with that gentleman, February 28.

"I am fully sensible, sir, of the honour done me by your concurrence in the conclusion drawn by Mr. Dircks; and I may be permitted to say, that all previous disappointment is amply compensated by your approval and final decision in my favour—conveyed too in a tone of warm eulogium, most gratifying, as coming from one to whose position and repute in the literature of the arts the highest respect pertains.

"In regard to Mr. Spencer, a few remarks may be expected from me: they must at present be brief, and will convey the intimation that I am as little disposed to view his labours in electro-metallurgy with jealousy, as I am inclined to relinquish my identification with its discovery. I await with some interest the advent of his promised documents, which are to annihilate all further rivalry—or otherwise. In either event I shall remain satisfied, being conscious of the difficulty he will encounter in extinguishing my claim to the character of an independent originator of this art, or in setting aside my incontestable priority in publication.

"I remain, very respectfully yours,

"C. J. JORDAN.

"March 8, 1844."

III.

MR. SPENCER,

In reply to the communication, page 57, wrote as follows:—

"Sir—I nowhere find in Mr. Dircks's last communication that *he* refuses the terms I proposed; but *you* do, in your editorial capacity, I suppose on his authority; and add, that "I am in no condition to stipulate terms." Indeed! And what was the gist of the proposed terms? That after Mr. Dircks had fully made his charge, I should reply—then submit both cases, after having been printed, to a competent committee, and whatever decision they came to, I should feel myself bound to abide by it—*and this is not accepted!* I said at the outset, with regard to Mr. Dircks, that I wished I had an opponent of whose honest intentions in the cause of truth I was better assured. I cannot help thinking that if I had had an honest opponent, there could have existed no possible difficulty in accepting such terms, they being, what all men desirous of settling a matter fairly, readily agree to. There is no man that I would have sooner left it to than yourself, from all I have hitherto seen of your conduct in the *Magazine*; but, sir, you can scarcely expect that now, seeing that you have already pronounced an opinion on the matter at issue, and that, too, from only one side of the question, and which (I speak with deference), if you had read carefully, warranted a very different conclusion.

"You further add, that it was easy for me to say when I claimed the invention—that I knew nothing of Mr. Jordan's letter, had never seen it, never read it, never heard of it. I assure you most solemnly I could have said so in the sincere spirit of truth, *if I had thought this gentleman's letter in any degree affected my claims*, after having read it the other day in your *Magazine*, for the first time.

"You also seem to be in much haste to see my reply —so does Mr. Dircks. I really do not see why I should withhold it now, unless it be to insure fair dealing, and perhaps let Mr. Dircks save me trouble—as he has half done already—which a careful analysis of his papers will show; however, let my reply come when it may, depend on me, I shall take care it has justice.

"For the second time I protest, in the strongest terms, against your allowing the *Magazine* to convey garbled statements of things that are quite foreign to the matter at issue; nor do I know of any thing more dishonest than that of quoting part of a statement, when the whole would convey a different meaning. I refer to the experiments I lately made on clairvoyance. A part of a paragraph, containing an account of one of them, was quoted by Mr. Dircks; but, had the whole been given, it would have conveyed a very different impression. As editor, you surely had it in your power to draw your pen through that which had no relation to the matter, more especially when I had remonstrated with you by private letter the first time it was alluded to. Should I send you a paper on the subject, I am sure you would not insert it, and you would be acting consistently with the interests of the *Magazine*. It therefore being a subject so foreign to your pages, it savoured something of partizanship to give a portion of it, obviously but to wound.

"I am, with much respect,

"THOMAS SPENCER.

"P.S. I am half inclined to think that from the beginning I have taken this matter up too seriously; the animus of Mr. Dircks must have been very apparent to all, yourself included; and yet, in every communication, you permit him to persist in making statements, and imputing motives, meant to be injurious to myself, and having no other foundation than his bare word. Really, sir, unless I see at least the semblance of fair play on your part, I must give the matter up in the pages of the *Mechanics' Magazine*.

"T. S."

[" We could," says the Editor of the *Mechanics' Magazine* in a note, " draw no other conclusion from the manner in which Mr. Dircks treated Mr. Spencer's proposition for a reference to 'a competent committee,' &c., than that he did reject it (though not, certainly, in express terms), and Mr. Dircks himself has not disputed the correctness of our conclusion. We are still of opinion that such a mode of proceeding was altogether uncalled for; and though Mr. Spencer may (affect to) wonder at this, we are sure nobody else does. Mr. Spencer finds fault with us for permitting Mr. Dircks to assail him in this way and the other on the strength of his 'bare word;' and threatens, 'really,' that 'unless he see at least the semblance of fair play on our part,' he must go elsewhere to obtain it. Mr. Spencer will do as he pleases in this respect; but we must distinctly deny that he has the slightest pretext for saying that he has not received fair play at our hands. He has been hit hard through the columns of the *Magazine*—that is true; but at the same time every opportunity has been promptly afforded to him of defending himself through the same medium—*if he could.* Mr. Dircks may have latterly diverged a little more from the main points, it is true, than is proper, and have used greater acerbity of language than it is pleasant to observe; but we could not but feel that the unwarrantably disparaging manner in which he was replied to by Mr. Spencer (who could find time for that, though he could not find a moment to bestow on the 'stubborn facts' of the case) gave him strong claims to indulgence."—Ed. *M. M.*]

CONTRIBUTION TOWARDS A HISTORY

OF

ELECTRO-METALLURGY.

(CONTINUED.)

On reading Mr. Spencer's preceding letter in the *Magazine* of March 16, my first impression was to treat his observations as utterly unworthy of notice, it seeming to be his object to write upon anything but the question at issue. On further reflection, I considered it might be worth while not to leave the subject quite in the unsettled state into which Mr. Spencer contrived to throw it. Writing on the 27th February, he pretends to believe I am going to republish my papers, "in all probability in the shape of a pamphlet," and thereon he makes a proposal, intended to act (as he says) "as a sort of wholesome check."

In my reply, 4th March, I say distinctly in alluding to the above, "Mr. Spencer sadly misunderstands me in my concluding remarks, in supposing I intend publishing a pamphlet on the present, to him, all-important discussion." Having thus clearly disclaimed all such intention of a pamphlet, it was not likely I could suppose that the "wholesome check" was to remain independent of every other consideration.

Mr. Spencer, however, proceeds—"And what was the gist of the proposed terms? That after Mr. Dircks had fully made his charge, I should reply—then submit both cases, after having been printed, to a competent committee, and whatever decision they came to, I should feel myself bound to abide by it—*and this is not accepted!*" Mr. Spencer puts this very triumphantly, but, with the foregoing explanation, it will be perceived that he was only shifting ground, making those terms general, which, originally, were only meant to apply in the event of my publishing my papers in the form of a pamphlet: a very gratuitous suggestion of his own.

To prevent all further cavil or misunderstanding, I replied on the 29th of March, stating that I have made, and consider I have, too, fully substantiated, my charge of plagiarism and want of originality; and was quite as willing as Mr. Spencer to abide by the decision of any committee competent to decide the question as to whether he is or is not the inventor of the electrotype process.

Here the matter dropped. Mr. Spencer neither in the *Mechanics' Magazine* nor any other Journal reasserted his alleged claim, or ventured on the publication of his supposed evidence of priority.

On the 18th of November, 1844, I again addressed the Editor of the *Mechanics' Magazine*, observing—

In *February* last, and in several subsequent numbers of your scientific miscellany, appeared my *Contributions towards a History of Electro-metallurgy*, proving that Mr. C. J. Jordan had precedence of Mr. T. Spencer in the discovery of the application of the deposition of metals by galvanic agency for various objects of art. Your readers, and yourself in particular, will remember that I was immediately addressed, through the medium of your journal, by Mr. Spencer, in no very measured terms, amounting to a declaration that I wrote from mere personal motives, and that he could, on abundant authority, prove his prior claim to being the originator of this beautiful and valuable art. Time only was wanting, and he intimated in decisive language his intention of setting the public right in the matter. Nearly nine months have now elapsed (March to November), and Mr. Spencer has made no effort, beyond the broad and unestablished assertions then put forth, to rebut my statements. But indeed, I may well ask, How could he? for I supported all my arguments by most irrefragable evidence.

In your review of Mr. Shaw's work on electro-metallurgy, you have shown that due credit is given by that author to Mr. Jordan; and I now beg to furnish the following remarks from Dr. Ure's excellent "Supplement to his *Dictionary of Arts, Manufactures, and Mines*, 1844;"

an extract which it will be perceived, as far as regards facts, is a brief summary of my paper before-named.

"ELECTRO-METALLURGY.—By this elegant art, perfectly exact copies of any object can be made in copper, silver, gold, and some other metals, through the agency of voltaic electricity. The earliest application of this kind seems to have been practised about ten years ago, by Mr. Bessemer, of Camden Town, London, who deposited a coating of copper on lead castings, so as to produce antique heads in relief, about three or four inches in size. He contented himself with forming a few such ornaments for his mantel-piece; and though he made no secret of his purpose, he published nothing on the subject. A letter of the 22nd May, 1839, written by Mr. J. C. (should be C. J.) Jordan, which appeared in the *Mechanics' Mag.* for *June* 8, following, contains the first printed notice of the manipulation requisite for obtaining electro-metallic casts; and TO THIS GENTLEMAN, THEREFORE, THE WORLD IS INDEBTED FOR THE FIRST DISCOVERY OF THIS NEW AND IMPORTANT APPLICATION OF SCIENCE TO THE USES OF LIFE.[*] It appears that Mr. Jordan had made his experiments in the preceding summer, and having become otherwise busily occupied, did not think of publishing till he observed a vague statement in the journals, that Professor Jacobi, of St.

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[*] I have purposely distinguished this handsome and merited compliment paid by Dr. Ure to the discoverer of electrography.

Petersburgh, had done something of the same kind. Mr. Jordan's apparatus consisted of a glass tube closed at one extremity with a plug of plaster of Paris, and nearly filled with a solution of sulphate of copper. This tube, and its contents, were immersed in a solution of common salt. A plate of copper was plunged in the cupreous solution, and was connected by means of a wire and solder with a zink plate dipped in the brine. A slow electric action was thus established through the moist plaster, and copper was deposited on the metal in a thin plate, corresponding to the former in smoothness and polish; so that when he used an engraved metal matrix, he obtained an impression of it by this electric agency. 'On detaching the precipitated metal,' says he, 'the most delicate and superficial markings, from the fine particles of powder used in polishing, to the deeper touches of a needle or graver, exhibited their correspondent impressions in relief with great fidelity. It is, therefore, evident that this principle will admit of improvement, and that casts and moulds may be obtained from those other metals having an electro-negative relation to the zink plate of the battery. With this view a common printing type was substituted for the copper plate, and treated in the same manner. This, also, was successful; the reduced copper coated that portion of the type immersed in the solution. This, when removed, was found to be a perfect matrix, and might be employed for the purpose of casting, where time is not an object.'

'Casts may probably be obtained from a plaster surface surrounding a plate of copper, &c.'

"On the 12th September following the above publication, Mr. Thomas Spencer read a paper 'On Voltaic Electricity applied to the purposes of Working in Metal,' before the Polytechnic Society of Liverpool; which he had intended to present to the British Association at Birmingham, in the preceding August; but not being well received there, he exhibited merely some electro-metallic casts which he had prepared. The society published Mr. Spencer's paper, and thereby served to give rapid diffusion to the practice of electro-metallurgy."

I omit the remaining three pages and a half of this interesting article, my only object being to show that we have here a second and high scientific authority adopting the opinion I promulgated, in regard to the history of this very scientific discovery, and which at the period of its publication gave much offence to Mr. Spencer. His refraining so long from fulfilling his promise of a refutation can neither arise from apathy nor indifference, and it must, therefore, appear evident to all who are acquainted with the circumstances of the case, that his silence implies assent to the correctness and justness of my early well-meant criticisms.

CONTRIBUTION TOWARDS A HISTORY

OF

ELECTRO-METALLURGY.

(CONTINUED.)

THE year 1844 passed away without any attempt on the part of Mr. Spencer to rebut the overwhelming evidence against him. But in the *Mechanics' Magazine* of July, 1844, in reviewing a second edition of "*A Manual of Electro-metallurgy;* by George Shaw," the following quotations occur:—

"The letter of Mr. Jordan, in the *Mechanics' Magazine,* attracted so little attention, that it had been overlooked; and probably would have remained unknown, had not Mr. Dircks, of Manchester, called attention to it in a recent number (1069) of that journal. It will be observed that Mr. Jordan anticipated all the most important parts of Mr. Spencer's communication, and must now take the position so long occupied by Mr. Spencer

—that of the author of the earliest successful application of electricity to the arts.

"The position which Mr. Spencer has so long occupied as the earliest English experimentor in this art, and which now clearly belongs to Mr. Jordan, cannot but excite the surprise of all who are interested in this art. It is surprising that the letter of Mr. Jordan to the *Mechanics' Magazine* should have been so long overlooked, and still more surprising that Mr. Jordan should have permitted the honour with which the origin of so important an art is associated to be appropriated by another. The supineness of Mr. Jordan is equalled by that of his friends, for it now appears that his claim to the origin of the art of electro-metallurgy was known to many individuals."

Some years elapsed before I had again occasion to notice this matter, when, in the *Expositor* of 24th of January, 1852, I made a communication, from which I shall now select the following extracts:—

Mr. Spencer, after a silence of nearly 8 years, has resuscitated his original claim, at a recent public dinner in Liverpool, reported in the *Liverpool Mercury*, Dec. 23, 1851. He there endeavours to establish a claim dating from the 9th of May, 1839, a fortnight before Mr. Jordan. This simplifies the whole matter in dispute, as to the priority of invention, between the respective candidates, and may be thus stated—

MR. SPENCER'S CASE.

He addressed the following letter to the then Secretary of the Liverpool Polytechnic Society[*] :—

[COPY.]

"May 9, 1839

"DEAR SIR—I wish it to be recorded upon the books of the Society that for the last 9 months I have been making occasional experiments on a method of executing any species of metallic device to any given pattern; and by the same agency have also been making experiments on a method of engraving in relief, both of which have been comparatively successful, and will, in my opinion, ultimately prove useful in the arts.

"I shall take an early opportunity of laying my process before the Society, with specimens produced by it. I should not have brought this subject before the Society thus early, but for the following passage, that appeared in last Saturday's *Athenæum*:—'Prof. Jacobi, at St. Petersburgh, has made a discovery which promises to be of importance to the arts: he has found a method (if we understand our informant rightly) of converting any line, however fine, into relief by a galvanic process; the Emperor of Russia having placed funds at his disposal, to enable him to perfect his discovery.'

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[*] The slightest attention to this letter will convince the reader how little it justifies Mr. Spencer's statement that in it he "mentioned some of the *results* of the discovery!" when, in the second paragraph, he says: "I shall take an early opportunity of laying my process before the Society;" that opportunity *not* occurring until the next September.

"That I have been engaged in this process the time above stated, is known to several gentlemen, who are now members of this Society.

"I am, yours respectfully,
"T. Spencer.

"Rev. T. Dwyer."

Nothing further appears on the journals of the Society before the next entry, viz.:—

"Thursday, Sept. 12, 1839.

"Mr. Spencer then proceeded to read a paper, giving an account of some experiments made to ascertain how far voltaic electricity might be usefully applied to the purposes of working in metal.

"Resolved—That Mr. Spencer's paper be printed at the expense of the Society, under the superintendence of the Council of the Society."

MR. JORDAN'S CASE.

On the 22nd of May, 1839, Mr. C. J. Jordan addressed a letter, on "Engraving by Galvanism," to the *Mechanics' Magazine*, which appeared in the number of that periodical for June 8, 1839.

In that letter Mr. Jordan describes the electrotypic apparatus—notices the usual deposits obtained by means of the earlier kinds of galvanic batteries*—also by the use of Daniell's sustaining battery, and Dr. Bird's modification of it (constituting, indeed, the single-cell process)—states on what depends the tenacity of electrotype deposit—specifies type-matrices, coins, dies, engravings, tubes, vessels, and other products; and, in short, fully, clearly, succinctly developes the entire electro-metallurgic process.

[This letter will be found at pages 3—6.]

REMARKS.

It will be observed that Mr. Spencer's letter of May 9 is so loosely worded as to be unintelligible, until we read the very last portion. During the discussion of 1844, this letter was never produced, *has never yet been made public by Mr. Spencer*, and is now (1852) published by myself for the first time. It will be acknowledged by those who read it, to be just such an undescriptive notice as any one—even ignorant of the subject itself—could have written; and yet, upon this precious "document" he attempts to establish a claim of priority in publication. It is no publication of any process, there being nothing in the shape of information about it. Mr. Spencer kept his secret from May 9 to Sept. 12, 1839 —during which time it might have been lost to the public (as far as he is concerned) by his death or other casualty. And it is remarkable that in the course of his speech at the before-named dinner, we find him admitting, in reference to his subsequent pamphlet, "it is to this that the public are indebted for their FIRST practical acquaintance with the electrotype"—ignoring, of course, the fact, that the public were made acquainted with the same, by Mr. Jordan, three months before!

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* It may be mentioned here, that this mode—known as the "battery process"—has been found to be, after all, the most economical, and, in manufacturing operations, is that almost universally resorted to.

In drawing up a parallel of Mr. Jordan's letter and Mr. Spencer's pamphlet, I brought to light an extraordinary instance of literary and scientific plagiarism, showing the entire pamphlet to be nothing more than Mr. Jordan's letter extended, elaborated, amplified, without an additional fact, and omitting one ingenious application through the typographical error of printing the word "tubs" for "tubes"—which latter appears to have baffled his research, notwithstanding the somewhat broad hint conveyed in the word "wire."

To give the title to priority of invention, there must be a first publishing of some kind—a sealed descriptive packet, open sale, printing, or any means that places the fact in the public possession. Such was *not* Mr. Spencer's case; and, however desirable for himself, he cannot disprove the fact that Mr. C. J. Jordan is the true originator of electro-metallurgy.

On reading the reports in London of the dinner given to Mr. Spencer, in Liverpool, I threw the substance of my former strictures on his pamphlet, in the *Mech. Mag.* of Feb. 17, 1839, into a parallel with Mr. Jordan's letter. This may not prove that he was not an independent inventor, but it does go far to prove that his invention was *not so early* complete as Mr. Jordan's, and that Mr. Jordan's communication was used by Mr. Spencer as the text-book for his pamphlet. This parallel of the two publications appeared in the *Liverpool Courier*, Dec. 31, 1851, as follows :—

THE ORIGIN OF ELECTRO-METALLURGY.

Sir—I have read in the *Morning Advertiser*, of the 24th instant, an account of a dinner given to Mr. Thomas Spencer, in Liverpool, representing him as the originator of the electrotype process; and he is reported to have made a speech animadverting upon certain statements made in the *Mechanics' Magazine* (1844), as tending to deprive him of that honour. Claims to original invention, whether in literature or science, can only be maintained and settled by a true and plain statement of facts; and as I was the first to establish the priority of Mr. C. J. Jordan's claim, I beg briefly to recapitulate those facts, to show how unjustifiable it was in Mr. Spencer to state that nothing "practical" had been published before his pamphlet. I shall now give a parallel of the two :—

ENGRAVING BY GALVANISM. BY C. J. JORDAN. *Mechanics' Magazine*, JUNE 8, 1839.	VOLTAIC ELECTRICITY APPLIED TO WORKING IN METAL. BY T. SPENCER. READ SEPT. 12, 1839.
Observing . . . . a notice, extracted from the *Athenæum*, relative to a discovery of Professor Jacobi, its perusal occasioned the recollection of some experiments performed about the commencement of last summer—	About four months ago a paragraph appeared in the *Athenæum*, stating that Professor Jacobi—7. It is two years since I began to experimentalise on this subject—7. In September, 1837,* I was induced to try some experiments in electro-chemistry—12.
—with a view of obtaining impressions from engraved copper plates, by the aid of galvanism, which led me to infer some analogy in principle with those of the Russian Professor.	I accordingly concluded that he was engaged in experiments analogous to my own—7.
—This phenomenon of metallic reduction is an essential feature in the action of sustaining batteries.	I had long been aware of what every one knows who uses a sustaining galvanic battery, that the copper plates acquire a coating of copper—13.
But the form of voltaic apparatus which exhibits this result in the most interesting manner—it consists of a glass tube—a plate of copper is placed in the solution, and is connected with a zinc plate—the result is precipitation of minutely crystallized copper, on the plate of that metal, in a state of greater or less malleability, according to the slowness or rapidity with which it is deposited.	A single pair of plates—a small piece of zinc, and an equal-sized piece of copper—12. I discovered that the solidity of the metallic deposition depended entirely on the weakness or intensity of the electro-chemical action—15.

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* Date of the British Association's first meeting in Liverpool

MR. JORDAN.	MR. SPENCER.
I remarked that the surface in contact with the plate equalled the latter in smoothness and polish, and mentioned this fact to some individuals of my acquaintance.	I then made mention of it to a few friends—7.
—on detaching the precipitated metal, the most delicate and superficial markings . . . exhibited their correspondent impressions.	On examining with a magnifying glass, I found every line was as perfect as the coin from which it was taken—19.
It is evident that casts and moulds may be obtained from any form of copper.	I was desirous of executing metallic ornaments, in either cameo or intaglio.
	First experiment, a very prominent medal—19.
This rendered it probable that impressions might be obtained from other metals having an electro-negative relation to the zinc of the battery.	Nor (I need hardly observe) is its application confined to copper only—8.
—a common printing-type was substituted—the reduced copper—when removed, was found to be a perfect matrix.	—in the manufacture of printing types—the matrix or mould—advantages appear to present themselves—9.
It appears, therefore, that this discovery may be turned to some practical account.	I feel convinced that it exhibits many promising indications of utility—9.
—a copper die may be formed from a cast of a coin or medal in silver, type-metal, or lead, &c.	—a means of procuring a cast or a die in hard metal—8.
	The applicability of this process in procuring *fac-similes* of coins or medals—9.
—which may be employed for striking impressions in soft metals.	I took a silver coin, and put it between two pieces of clean sheet lead, and placed them under a screw press. From the softness of the lead, I had a sharp mould—20.

MR. JORDAN.	MR. SPENCER.
Casts may probably be obtained from a plaster surface surrounding a plate of copper;	—to deposit a metallic surface on a model of clay or other non-metallic body—21.
—tubs or any small vessels may also be made by precipitating the metal around a wire, or any kind of surface, to form the interior—	[For "tubs" read "tubes" —a typographical error in the original letter. Mr. Spencer did not attempt *tubs*. He considered it impracticable to make copper "vessels"—ships !]
which may be removed mechanically, by the aid of an acid solvent, or by heat.	—easily separate . . . by the application of heat. — When the heat of a spirit lamp was applied—the impression fell easily off—20.

I leave it to the impartial reader to say whether he considers these numerous coincidences accidental, or as the result of an intimate acquaintance with Mr. Jordan's letter.

Mr. Spencer, when writing to the *Mechanics' Magazine*, in Feb., 1844, made great profession of possessing evidence which he could adduce in support of his disputed claim; but, though promised, it was never produced. This blank naturally left it doubtful whether there might not be some fact in his favour which had been overlooked, or even interestedly suppressed. Mr. Spencer, however, made the occasion of the "public dinner," given to him by his private friends in Liverpool (see the *Liverpool Mercury*, Dec. 23, 1851) the medium of conveying to the public, in a long and carefully written speech, read on that occasion, his own version of that so-called documentary and other evidence* which he has thus pertinaciously withheld for nearly eight years.[*] For this extraordinary proceeding, with so many chances of time causing those losses in evidence, which fortunately have not occurred, even Mr. Spencer's own apology can scarcely be taken as a sufficient excuse for breaking his promise recorded, as above stated, in 1844, thus laying himself open to the suspicion of wishing to strengthen his case through defective evidence against him.

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* Pp. 25, 26.

However, we shall wade through his desultory oration for those "facts and dates" we are there promised.

But first for his apology, when he says—"Let me not be accused of egotism in thus distinctly stating the particulars and dates of this discovery. I have been so apprehensive of such a construction, that I have hitherto forborne to give public denials to those statements to which I have referred until this evening." Egotism! Is it any egotism to defend just claims? Is it any egotism to fulfil promises written, printed, and published? Surely, it is much more egotistical to promulgate hollow pretensions. Mr. Spencer does not appear to have been animated by any such delicately scrupulous aversion to egotism on many other occasions.

The statements which he here alludes to are charges affecting his claim, occurring—first, at Glasgow;[†] secondly, at Liverpool, in 1841; and thirdly, in the *Mechanics' Magazine*, 1844—this last being, in his own words, "to the effect that I had copied my experiments from a letter written by a Mr. Jordan, and published, I think, in the June number of that print for 1839. Nothing could be more absurd than this last charge; in fact, I had never seen the letter until it was thus pointed out; and, if I had, I COULD NOT HAVE OBTAINED ANYTHING PRACTICAL FROM IT. It referred to the experiments of Prof. Jacobi; and the author stated, that in making galvanic experiments similar appearancs occurred to himself, which I dare say was very true. Even the editor of the *Magazine*, though leaning to his correspondent, had the candour to admit that even he had not seen the letter, though occurring in his own publication."

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[*] That is, from Feb., 1844, to December, 1851.

[†] Mr. Spencer should, by all means, while adhering to "facts and dates," have stated that he here alludes to the Glasgow Meeting of the

Nothing can be fairer than to let Mr. Spencer thus speak for himself. Now to answer him:—

1. My charge was not that he had copied his "experiments" from a letter by Mr. Jordan (however true it may be), but that he had made that letter the basis, the

---

British Association, in 1840, when his opponent was no other than Professor Jacobi himself. Dr. Thomas Thompson, F.R.S., who presided over the Chemical Section, was obliged to interpose his authority to check Mr. Spencer's exuberant warmth in urging his own claims. Professor Jacobi's invention of galvano-plastics, or electro-type, it was stated, had been first recorded in a notice to the Petersburgh Academy, on the 5th of October, 1838; and in a Petersburgh journal, 30th Oct., 1838. See also a notice of his experiments in Sturgeon's *Annals of Electricity*, vol. iii., July to April, 1839.

skeleton of his own pamphlet, and had said nothing beyond Mr. Jordan.

2. Mr. Spencer says "he had never seen the letter" till 1844. This assertion is strange, but of no account. I have before stated, and now again seriously, considerately, and deliberately repeat, that when I mentioned to him having seen the letter on its publication, and named some of Mr. Jordan's results, he then spoke of it familiarly, as perfectly acquainted with the letter; and which I could not doubt to be the case, as he was a reader of the *Mechanics' Magazine*, and had the reputation of great familiarity with scientific periodicals generally. He said *the letter did not affect him*, and I took him at his word at the time. Perhaps the parallel given at pp. 86—88 will throw additional light on this matter, and may surprise all who find two inventors following each other so closely, particularly when one writes four months after the other. IF Mr. Spencer was ignorant of Mr. Jordan's letter, in spite of our conversation at one of the Societies in Liverpool, his own pamphlet must henceforth be considered as one of the most astonishing instances on record of two individuals, 200 miles apart, experimenting, thinking, and writing distinct productions, in an almost uniform coincident strain. Truly does he say—"duplicate discovery is but seldom the result of mere chance alone."

3. As to whether he could "have obtained anything practical" from the letter of Mr. Jordan, the unpre-

judiced and scientific reader can best judge by referring to it again, at pages 3—6. Mr. Spencer is either very willing to be suspected of an exceedingly dull comprehension, or must suppose the scientific world are to be blinded by any statements, made with sufficient assurance and boldness, to dare thus to depreciate and stigmatize Mr. Jordan's efforts.

4. His outline of Mr. Jordan's truly lucid exposition of the science of electro-metallurgy, in its very commencement, and before the much vaunted enlightenment afforded to the world by his own pamphlet in Sept. following, is meagre, unjust, and entirely false; but it is an excellent comment on his own empty notice of May 9, 1839 (p. 81). This constant gross depreciation, whenever Mr. Spencer alludes to Mr. Jordan, marks his every statement—a common indication of a weak and indefensible cause.

5. The editor's absence from town, and his functions being temporarily in other hands, was the real cause of the oversight which Mr. Spencer has the effrontery to turn into an insinuation expressive of the utter insignificance of Mr. Jordan's ingenious communication, as if even the editor himself "had the candour to admit," &c. Certainly, "nothing can be more absurd than this!"

Fortunately, Mr. Jordan's position receives neither accession nor diminution from Mr. Spencer's pretended ignorance or knowledge of his letter. It is sustained by its intrinsic merits. Mr. Jordan had published when Mr. Spencer had NOT published — a fact decisive on the question. But we will listen to Mr. Spencer, who says: "My claims have been usually admitted by those who have written on the subject, as dating from May 9th, 1839; because, at a meeting of the Polytechnic Society, held on that day, a letter was read from me to the Secretary, bearing the date of May 6.* This letter, which is entered on the books of the Society, mentioned some of the results of the discovery, and also that I had been engaged in perfecting (!) the process for a considerable period." Mr. Spencer should by all means have read this letter at the dinner—a letter so important, and containing, too, "some of the results of the discovery," so far back as May 6, 1839, affording evidence (as the unwary might suppose) of a prior claim! But no; although this letter is, as it now appears, his mainstay, yet neither in the controversy of 1844, nor at the dinner of 1851, where he professes to give "those facts and dates" which have connected his name with the early history of this art, was this great "fact" ever before produced! This, then, is the letter, the mention of which has duped scientific writers from 1839 to 1844, and by which it is even now attempted to deprive Mr. Jordan of the distinguished honour of being the inventor of the electrotype. (See the letter, p. 81.)

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* See page 33. May 8 has been the date usually quoted. The correct date appears to have been May 9; see page 81.

It will be here noticed how Mr. Spencer colours and endeavours to render important this forestalling intimation, suggested by Prof. Jacobi's previous notice, of which it is a mere counterpart. It is not a "communication," a sealed packet containing a descriptive account of his entire process up to that period. It is—nothing; and all such notices go for nothing, in all scientific matters, in deciding the rights of independent inventors —he alone ranking as the first and true inventor, who, by a "communication," first places the public directly, or in trust, in actual possession of the invention claimed. Mr. Spencer did nothing of this kind till Sept. 12, 1839. Had he died previous to that date, *his* secret would have been lost. Mr. Jordan wrote May 22, and his letter was not merely a *notice*, but literally a "communication"—complete, explicit, and unreserved. And this conclusive fact will be admitted by the universal assent of an impartial and discerning public.

Mr. Spencer adopts, in a large portion of his speech, a system of accumulative evidence, as if confidently hoping that proofs of early engagements in electrical pursuits would strengthen the claim he has set up in connection with electro-metallurgy. Thus, he read a paper Feb. 12, 1839, on "The theory of the formation of metalliferous veins, by galvanic agency, in the interior of the earth," when he "succeeded in obtaining specimens of pure crystalline copper." Then, again, his friends see his specimens; and so he proceeds, with an amusingly tortuous ingenuity, to assert a right to date his "discovery" anterior to May 9, 1839—observing: "Now, *the date* of this paper carries my claims to priority very much further back than I have hitherto deemed it at all necessary to go!" This kind of reasoning, among partial personal friends, met for conviviality, may be received by them with considerable approval; but I will not insult the scientific understanding or the common sense of my readers, by the refutation of such special pleading. Certainly, not even the tyro in chemistry, much less any practical operator in electro-metallurgy, however deficient in scientific history, will treat otherwise than with contempt such vain bombastic claims as these to an invention which Mr. Spencer carefully, cautiously, and with a prospective object, declares, on the 9th of May, 1839, was even then only "partially successful!"

I contend, therefore, that this latter course of proceeding is quite untenable, opening, as it does, a wide field for dispute, both between the present parties as well as others who might become claimants on similar grounds. It looks, too, like the last resort in a dying cause. Besides, Mr. Spencer, while pleading his own case, with singular fatuity leaves his audience to take for granted that Mr. Jordan could *not* prove as much—conveniently forgetting, likewise, that Mr. Jordan himself, in his very letter, refers to a period "early in the summer." But I deny the right of producing such doubtful evidence against accredited evidence. Admitting, on his own showing, Mr. Spencer's originality and independence as an inventor, just as I have proved Mr. Jordan to be original and independent—how stands the case? We have two inventors, operating under similar circumstances, stimulated to print by the same cause, one writing a communication May 22, 1839, the other writing a *notice*, merely paraphrasing the vague rumour about Jacobi's experiments, dated May 9, 1839, and subsequently reading a communication to a Society Sept. 12—nearly four months after Mr. Jordan.

I have thus brought my examination of this subject to a close, and I trust that, although much longer than really necessary as regards the mere facts, yet, that it will prove more generally satisfactory by being thus submitted in its original form.

The future historian of that now extensive branch of art generally denominated electro-metallurgy, or electrotype, and better deserving to be denominated Jordantype, dating from June, 1839, will find his first step in his labours facilitated by this early, full, and most satisfactory sifting of the subject—one which, in 1844, possessed all the materials for future interminable discussions, beset with intricacy and mystery; but on which various circumstances have fortunately contributed to afford the most conclusive arguments respecting its true origin.

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